ADELAIDE

PHILOSOPHICAL SOCIETY.

ANNUAL REPORT AND TRANSACTIONS

FOR THE YEAR ENDING 30TH SEPTEMBER, 1865.

OFFICERS FOR THE PAST YEAR:—

PRESIDENT:

His Excellency Sir Dominick Daly.

VICE-PRESIDENTS:

His Honór R. D. Hanson, Esq. | Charles Todd, Esq.

REPRESENTATIVE MEMBER OF THE BOARD OF GOVERNORS:

W. Wyatt, Esq., M.D.

TREASURER:

John Howard Clark, Esq.

HONORARY SECRETARY:

Mr. James Hosking.

ADELAIDE:
ANDREWS, THOMAS, AND CLARK, PRINTERS, REGISTER GENERAL PRINTING OFFICE, GRENFELL STREET.

1865.

Annual Report of the Council of the Adelaide Philosophical Society

FOR 1864-5.

"Your Council have now the pleasure to place before you their annual report (being the 12th from the formation of the Society), and embracing a period from October 18, 1864, to the present time.

"Within the year six ordinary members and one honorary corresponding member (the late Geo. Windsor Earl, Esq.) have been elected.

"Your Council regret to have to announce in the same report which officially records his election as an honorary member of this Society the death of Mr. Earl, on the 9th of August, on board the Shantung, when on the point of sailing for England. It will be remembered that Mr. Earl visited Adelaide in 1864, and returned to Penang at the end of the same year; it was hoped with renewed health and energies, to be for many more years devoted to the cause of science. During his stay amongst us he formed many lasting friendships, and was ever ready to aid any movement calculated to promote the material prosperity of the colony. He also took an active interest in the settlement of the Northern Territory, in connection with which his long residence at Port Essington enabled him to give much valuable and timely information and advice to the Government, then engaged in fitting out the first expedition under Colonel Finniss. His last contribution to this Society was a letter, addressed to one of your Vice-Presidents, Mr. Todd, on the meteorology of Northern Australia, which was read at the September meeting.

"Your Council have the satisfaction of stating that the monthly meetings held during the past year have been well attended. The following papers have been read, viz.:—

* 'Wool-Growing in North Australia,' by R. Wells, Esq.

'The Papillionidæ of Celebes,' by C. A. Wilson, Esq.

* 'The Extraction of Scents from Plants,' by the Hon. S. Davenport, M.L.C.

* 'The Comet,' by Charles Todd, Esq.

'The Tertiary Rocks of South Australia,' by the Rev. Julian E. T. Woods, of Penola:—

* No. 1—Introduction.

* No. 2—The Mount Gambier Fossils.

* No. 3—Brachiopoda.

'Notes on the Darwinian Theory,' by C. A. Wilson, Esq.

* 'The Origin of Language,' by His Honor the Chief Justice.

* 'The Drainage of Adelaide, considered in its Scientific Aspects,' by the Rev. J. Maughan.

* 'Meteorology of North Australia,' by Geo. W. Earl, Esq.

'The Results of Meteorological Observations made by Mr. Jacob Bauer at Adam Bay, North Australia,' by Mr. C. Todd.

"Descriptive addresses have also been given by several of the members on a variety of interesting topics, viz.—

'On Specimens of Natural History from the Northern Territory,' by F. G. Waterhouse, Esq.

'On Forest Trees,' by C. S. Hare, Esq.

'The Construction of the Spectroscope,' by A. S. Clark, Esq., &c., &c.

"Several books and pamphlets have been presented to your Society by authors and scientific bodies; and that these with the Society's other books and papers may be read by the members during the ensuing year, your Council have instructed your Hon. Secretary to get them arranged and put into circulation.

"In accordance with the expressed wish of the Society, the sum of £15 was remitted last May to London for the purchase of a few back volumes of the Transactions of the Royal Society. In place of executing the order, however, your Society's Agent forwarded to your Council an offer of a second-hand set of volumes, comprising the Transactions from 1850 to 1864, and an abstract of the Transactions from 1800 to 1850, for about £35, including binding. As it was necessary to forward a reply by return mail, your Council assumed the responsibility of accepting this offer, and a further sum of £20 has been accordingly remitted. The payment of this amount does not appear in the Treasurer's account presented this evening, as it falls within the new financial year; but owing to the fact that some of the members are still in arrear with their subscriptions, its effect is to overdraw the Society's account with the Treasurer to a small extent. Your Council trust that these members will promptly discharge their obligations to the Society, and not add to the labours of your Hon. Secretary, by causing him unnecessary trouble in the collection of the subscriptions. In reference to this subject, your Council would draw the Society's attention to the desirability of curtailing in some measure the current expenses, which have been increased of late by the arrangement for printing the papers read before the Society."

JAMES HOSKING, Hon. Secretary.

The Papers marked thus (*) are printed in the annexed transactions.

THE TREASURER IN ACCOUNT CURRENT WITH THE ADELAIDE PHILOSOPHICAL SOCIETY.

Dr.

1864.	
October 1—To Balance in hand as per last account	£44 7 2
1865.	
September 30—To Arrears of Subscription received	15 15 0
" " Current Year's Subscriptions received ...	40 19 0
	101 1 2

Cr.

1865.	
September 30—By Expenses incurred to date, viz:—	
Contribution to South Australian Institute for the year ending 30th Sept., 1865, one third of £56 14s.	£18 18 0
Advertising Meetings	7 6 9
Printing Circulars (two years)	12 9 6
Receipt Books, Stationery, &c.	4 2 6
Coffee, &c., at meetings (eighteen months) ...	15 19 0
Postages and Petty Expenses	1 19 9
Printing Society's Papers and Lithographing Drawings	10 12 0
Remittance to London on account of purchase of Royal Society's Transactions ...	15 0 0
	86 7 6
Balance	14 13 8
	101 1 2

October 1, 1865—To Balance in hand £14 13 8

JOHN HOWARD CLARK, Treasurer.

Audited and found correct,

J. Z. SELLAR.

ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER READ ON TUESDAY, OCTOBER 18, 1864, BY R. WELLS, ESQ.

SUBJECT—"WOOL-GROWING IN NORTH AUSTRALIA."

Mr. WELLS said he should have to refer to Mr. Earl's paper, because that gentleman's views on the subject of wool-growing in North Australia appeared to have undergone a great change. His first work on "Tropical Australia" had recently been quoted in England to support the idea that North Australia was a country altogether unsuited to the sheepfarmer. In that work, which was published 17 or 18 years ago, it was said that the sheep taken to Port Essington from New South Wales did not thrive; and the writer then went on to say—"Even if it be found that other pasturages are better suited to them, the *nature of the climate generally* forbids the hope that wool—the staple export from the southern colonies—can ever be produced with advantage in the *tropical* regions. The c untry at the head of the Gulf of Carpentaria and the hills that abut upon the north coast, which from their peculiar position enjoy a lower temperature than other parts, may prove to be adapted for sheepfarming. But the heat and moisture of the tropics, so highly favourable to vegetation, point out the produce of the soil as that to which future settlers must look forward as the reward for their toil." Now as this quotation and the remarks on the falling-off observable in the sheep which were taken to Port Essington had been used and commented upon in England, it was lucky that Mr. Earl, in his recent paper, had explained that what he said in 1846 must now be accepted in a considerably modified form. His present remarks were—"Upon the whole, I think that sheeps' wool may safely be included in the list of future products of the tropical region, although I am of opinion that its production will be confined to the open country of the inland districts. Not that the coast region is altogether unfavourable to its growth as far as climate is concerned, for the open grassy plains which border the streams of the Coburg Peninsula were so chilly during clear nights that exploring parties always preferred camping out in the woods at their borders, although the distance from water was sometimes as great as a quarter of a mile." From this it appears that the writer had not only modified his opinions as to the wool-growing capabilities of a *tropical* country generally, but that he now believed the "inland districts" of North Australia would be found most suited to this purpose; wherea, in 1846 he thought that if there was any sheep-grazing country at all in the north it would be found upon the hills that abut upon the coast. Mr. Earl's latest view of the case was an important one to Australian sheep-farmers; and the reason why special attention should be drawn to it was that the writer was justly regarded as a high authority on the subject. It was possible, therefore, that his present opinions might counteract the misapprehension to which his previous statements partly gave rise. Of course, when Mr. Earl wrote in 1846 scarcely anything had been done to put this question to the test. But since that time sheep had been depastured in various parts of tropical Australia, and the general result had been favourable. This, however, did not appear to be known in England, and people were therefore clinging to the old idea. They believed that the experiment at Port Essington had been decisive, and that there was no evidence as to the practicability of sheepfarming in tropical Australia. As to the value of the information which was really possessed on the subject, it went far to prove that North Australia might soon be a great wool-growing country. According to theory it ought not to be so, perhaps; and that was the reason why so many objections were raised to the facts brought forward. At a meeting of the Royal Geographical Society which took place in August last year, Mr. Alfred Wallace, a gentleman who had lived in India, and whose name was well known, said, during a discussion in reference to the capabilities of North Australia, that the district which it was proposed to colonize by South Australia was not only tropical, but almost equatorial in its character, the Victoria River being in 15° south latitude. He did not believe they would find any country in the world within 15° of the equator in which the European wool-bearing sheep could exist; consequently, the colonists who went to that part of the country with the intention of commencing sheepfarming would be exceedingly disappointed, because even in the more favourable island of Timor, which closely resembled Australia in its physical characteristics, the sheep had no wool. Sheep brought from Australia for the purpose of experiment began to lose their wool after they had been there a year; and in a tropical climate like that they made no fat, which was the only other commodity for which sheep were valuable. Therefore, said Mr. Wallace, "if the wool turned to hair, and if the fat went away, he did not see how sheepfarming could be carried on with success" A similar opinion to this was expressed at the same meeting by Mr. Crawford, who spoke decisively as to the unsuitability of the climate to the pasturing of sheep. These gentlemen, in fact, seemed to be entirely of the opinion which Mr. Earl expressed in his "Tropical Australia"—that the soil would produce almost anything, but that wool-bearing animals would deteriorate from the moment of their introduction. Now in opposition to these theories, there existed a few facts, which if not conclusive were certainly important. In the first place, Mr. Wallace's statement that European wool bearing sheep could not exist within the 15th degree of south latitude is met by the fact that flocks of sheep had for a long time been depasturing within that latitude. Statements had been received from Queensland, showing that upon runs lying in about the same latitude as that of the Victoria River sheepfarming was being carried on with success and that no deterioration had been observed in the quality of the wool which was there produced. It had been remarked, indeed, on this point, by Sir Charles Nicholson, that at this very time "there are upwards of a million of sheep in the highest possible condition being pastured within the tropics." Another fact of some little importance was that Mr. Augustus Gregory took sheep with him to the Victoria River, which is in latitude 15°, and kept them there for many months during his exploration of the country in 1856. He observed no falling-off in the state of the animals; and in his report he spoke in the highest terms of the country as a pastoral region. But there was a more recent case than this in the experience of Mr. McKinlay, who took sheep from South Australia to the Gulf of Carpentaria without any inconvenience; and certainly the wool of these did not turn to hair, for Mr. McKinlay brought back the fleece of several of them, in order to show that the climate had produced no injurious effects. Mr. McKinlay, writing on this subject, said—"I perceived no difference in the quality of the wool; but knowing the impression on some minds that wool suffered injury in hot climates, I was the more particular in satisfying myself that such was not the case by bringing back with me two of the sheepskins, which may now be seen in the office of the Crown Lands Commissioner." This statement was certainly useful as far as it went. The facts, then, showed that wool was now produced in tropical Australia, and that the northern part of Queensland was becoming every day more and more occupied by sheep. The theories which had been brought forward in objection to this were met by plain facts; and, what was more, there were able men of science refuting the theorists with their own weapons. Sir Roderick Murchison, in answer to the objectors just referred to, had said—"They speak from their experience of India and of the Eastern Archipelago. But they forget the law of isothermal lines. They forget that India is subtended by the ocean, which conveys heat to lands in the latitude of Madras, while in the very same degree of latitude south of the equator there are great tabular masses of land which temper the heat. We have in this physical fact the explanation why sheep and other animals flourish in that latitude in Australia." The practical bearing of this question was one of no small importance to the sheepfarmers of these colonies, especially to those of Queensland. With regard to ourselves, the difficulty was how to convey flocks of sheep to North Australia. According to all reliable evidence, it would be a great trouble, if not an impossibility, to do this by way of Stuart's route, for the chance of finding sufficient water at suitable places through Central Australia would be very small. As to the country which Mr. McKinlay passed through, there was less difficulty, but unfortunately the Government of Queensland had thought proper to prohibit the passage of flocks and herds by that route. This most arbitrary act was much to be regretted. It had shut out the sheepfarmers of South Australia from the northern country, and had taken from them the opportunity of at once testing the adaptability of the new territory to the growth of wool. It was certain, however, that the experiment would soon be tried.

Mr. MAGAREY would like to know what sort of sheep were taken to Timor. He knew by experience that at Rockhampton, in Queensland, the climate was very hot, but he believed no change was observable in the sheep depastured there—at least no change that could not be accounted for by other causes.

Mr. CLARK did not understand that in any part of Queensland sheep was kept so far north as the country which was now proposed to be settled.

Mr. WELLS remarked that the sheep taken to Timor were spoken of as "European wool-growing sheep;" and that with regard to Queensland, the Gilbert, the Lynd, and the Mitchell were as far north as the Victoria, yet portions of the country were said to be successfully occupied by sheep-farmers. Wool had long been grown in some parts of India.

Dr. NOTT believed that it was found desirable to constantly import new blood in Queensland for the purpose of preventing any deterioration.

The CHAIRMAN urged the desirability of sheep being sent even by sea as early as possible to the northern settlement, in order that the question might be fairly tested. He also hoped that steps would be taken to secure constant and accurate meteorological returns in reference to the climate in the neighbourhood of the Adelaide River.

W. K. Thomas, Printer, Register Steam Printing Office, Grenfell Street, Adelaide.

ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER READ NOVEMBER 15, 1864, BY THE HON. SAMUEL DAVENPORT.

Subject--"The Extraction of Scents from Plants."

"Since the cultivation and development of our faculties is both an aim and sign of civilization, it may be admitted to the sense of smelling to have some claim on our attention. It is said that its cultivation, like that of other senses, augments its powers, and that persons given to the due use of perfumes have the faculty of smelling rendered more acute. Perfumes are donations from the vegetable kingdom chiefly. They constitute a store, given by Providence to the charge of plants, whence man may be supplied with refreshment and health, obtainable through the medium of the organs of smell or taste. The perfumes of plants are antiseptic, stimulating, disinfecting midst vitiated atmospheres, often tonic, and as gratefully invigorating the sensations of life as the beauty of many flowers and plants producing them is pleasing to the eye. They constitute the special attraction of perfumery—bouquets and nosegays, smelling-salts and vinegars, sachet-powders and incense, scented soaps, pomades, and oils, &c., as well as of numerous liqueurs and the most *recherché* confections. Thus they also possess a commercial value.

"The ancient pagan world acknowledged the relative eminence of their regard for perfumes by the offer of costly incense to the gods of their temples; and on the dawn of Christianity the same medium of reverence for Deity is traceable in the offering to the infant Saviour of gold, frankincense, and myrrh. Subsequently, through various phases of human existence, perfumes have entered into exhibitions of devotion, be it in the temple service or the social feast, the pomp of royalty, or the quiet luxury of private life. In England, according to the author of the 'Lives of the Queens of England,' perfumes were never richer, more elaborate, more costly, or more delicate than in the reign of Queen Elizabeth. She had a cloak of perfumed Spanish leather, and in her days perfumed gloves and shoes were fashionable. Whilst at Hawkstead, among the rooms on the ground floor was one called the 'stillroom,' where ladies of the Court amused themselves in distilling fragrant waters; and the ladies of the wealthy classes generally in those days took lessons in the art of preparing perfumes and washes. But to take the world of the present day, and its demands for perfumes of plants, some of the fairest spots of Europe and Asia are devoted to their cultivation; and the question brought home to us is whether this, our own sunny soil of South Australia, should not join competitor with these other fair spots of their production, nor leave the fine fragrance of its flowers to be longer wasted on the wandering air. Great Britain pays annually to foreigners for perfumes half a million of money, because none of her own colonies as yet supply her wants.

"The perfume or aroma of plants resides in their essential or volatile oils. These oils exist in various parts of plants. Sometimes they are distributed throughout the plant, as in the Angelica, often in the leaves and branches, as in balm, mint, and wormwood (absinthe). The Florence Iris and ginger retain their essential oils in the root only; whilst lavender, thyme, rosemary, and verbena have theirs in the leaves; the rose, the jasmine, the heliotrope, the citron, and orange in the flower; and the last two also in the leaves and skin of the fruit. Aniseed and fennel contain their oil in small vessels ranged along the salient lines observable on the outer bark. Other plants carry their oil in the bark itself, as cinnamon; in the substance of the wood, as sandalwood and myall; in the seed, as coriander and caraway.

"Essential oils are distinguishable from fatty and other oils by their volatility and by their property of not leaving spots on paper. They have an acrid or burning taste, and are colourless or variously coloured—as yellow, red, brown, green, or blue. They are lighter than water, excepting the oils of cinnamon, cloves, sassafras, and mustard. They are generally fluid, and evaporate in the air at its ordinary temperature, and more readily under heat; but all can be solidified by reduction of temperature, and some—as the otto or essential oil of roses—readily congeal. They ignite when in contact with fire, and burn with a clear flame, but much smoke. Distilled pure, many of them decompose; but with water added in distillation, their properties remain unchanged. Though coloured when extracted, exposure to the air darkens them through the absorption of oxygen. Light assists this change; and, under the joint influence of light and air, the oil thickens, loses its odour, and grows resinous. Therefore, to preserve essential oils they should be enclosed in glass-stoppered bottles, perfectly filled, and kept in a dark chamber, or coverd with black paper. These essential oils are slightly soluble in water, readily so in alcohol, the more as it is concentrated, whilst ether dissolves all essential oils. They combine with certain acids, as acetic and oxalic acids, and they dissolve in resins and animal fats. Essential oils are much used for medicines, for perfumery, and for varnishes, because of their properties of dissolving colours, and their rapid evaporation on being applied.

"The maximum of oil, both in quantity and quality, is yielded when the parts of the plant containing them arrive at full maturity. Thus roots yield best at the end of spring; leaves and branches when the flower is on the point of development; the flower at the moment of its full expansion; and the fruit when first ripe. The most of plants, and especially flowers, give off essential oil only when fresh; some will preserve their oil for years uninjured, as the milfoil and balsam, which yield oil only when dry. The quantity of oil for extraction from plants varies with the kind, the climate, soil, exposure, freshness, or dryness. In general, those plants which grow spontaneously on arid soils, hilly and exposed to the sun, furnish the most volatile oil, especially from the flowers when they first expand, and if distilled as soon as gathered.

"The extraction of perfumes, scents, or the essential oil of plants is accomplished in one of the following ways:—First, and the most natural, by expression. With your hand you squeeze the fresh peel of an orange against a burning candle, and the jets of essential oil emitted ignite with a species of explosion on contact with the flame. By like squeezes you may eject it into a sponge till that is saturated with the essential oil of orange-peel, known in commerce under the name of 'Eau de Portugal'—Portugal water. Of course, however, in a manufactory of scents, machinery is substituted for the fingers, and the peel is subjected to a strong screw, or hydraulic press, acting on a cylinder furnished with a false bottom perforated with holes, through which the expressed oil falls, and is gathered by means of an orifice made at the base of the cylinder; but here only that substance of the peel is taken in which the small vesicles containing the oil lie, and hence the epiderm, or true outside rind, is pared or grated off from the white and interior stratum of the peel, and the pulp of this alone is submitted to the press. In Italy, Portugal, and Provence, the pulp thus resulting is expressed between two glasses slightly inclined. The produce, after resting awhile, deposits a little parenchyme or fleshy fibre, from which, when filtered through paper, the oil is passed into a well-stoppered bottle.

"But this and other essential oils are obtained also by the process of distillation, the chief of all extracting processes. Two classes of products result from distillation—the oil or scent pure, and the water saturated with the oil, called distilled water. But, first of the oils. When the parts of odoriferous plants are distilled with water, the oil passes over along with the steam, and separates itself in a great degree when condensed in the receiver. The distilling apparatus used resembles that employed for converting wine into brandy, excepting that the still is higher and presents a lesser surface to the fire. The head and worm of the still are often of tin, and the still tin-lined, in order to avoid imparting a coppery odour to the products. The process of distilling is as follows: The plants, or parts of plants, are placed in the boiler. Water is then added, the head put on, the worm attached, and the receiver put in place. The joints are then luted and the fire lighted. As the steam of the boiling water rises, the essential oils, also set at liberty by the same heat, pass through the head into the worm, where, condensing under the cooler temperature, they jointly flow out in a limpid state into the receiver. Soon that water is troubled, becoming milky by the separation from it of the oily particles, which, being lighter than the water, mount to the surface, and there form a bed increasing in thickness as the distillation proceeds, and which, drawn off, is the oil in a state of purity. A writer in the 'Maison Rustique' says that in the South of France, where the greater part of the essential oils of commerce are prepared, the makers of them, in the month of July, transport their stills to the localities where a sufficient quantity of aromatic plants exists, and set them up in the open air, as near as possible to a running stream. By means of four stones they construct a furnace, and announce their demands for this or that plant. The inhabitants of the neighbourhood arrive, agree upon the price to be paid them for their plants, which, being gathered and distilled, the manufacturers move on to repeat the process elsewhere.

"The quantity of water which should be added to a plant in the still varies according to the kind of plant and the amount of oil that plant is capable of yielding. If too much water be used, the essential oil being to a certain degree soluble in water, less oil is obtained, and the product is only water saturated with oil. On the other hand, if too small a quantity of water be used, the plant will adhere to the bottom of the boiler, and give a burnt flavour, materially altering the quality of the oil produced. Experience alone must guide the manufacturer; but it is well rather to exceed than to be too sparing of the quantity of water used. The water in the receiver may be again employed in the still, so that the oil it holds in solution be thus less subjected to loss.

"To procure the best quality of oil the first formed is drawn off from the receiver before the process of distillation is complete, the first formed being the strongest and most agreeable odour. Some oils being less volatile than others, and therefore passing off less readily under distillation, are aided by the addition of salt to the water used in the still, the effect being to augment its density, and to require a higher degree of heat for the boiling point, which increased heat favours the volatilization of the oil. The water of the cooler generally separates more oil when it is kept at a

low temperature; but as some oils are solid at a low temperature it is necessary in their cases to avoid reducing the water of the cooler to a less degree than 45° to 50° Fahrenheit, or the oil will remain solidified in the worm. Of this class is the oil or otto of roses. The fire should be kept at a moderate and regular heat, not subjected to great alternations, as when forced by bellows or drafts of air, first exciting to greater life, then relapsing its vigour; for extreme alternations of heat applied tend to burn the plants and to decompose them, to the injury of their production of oil.

"You cease to distil when the product becomes insipid and inodorous. The receiver in common use in the extraction of essential oils in the South of Europe is the Florence receiver ('Recipient florentin'), a kind of flask, having a tube rising from its base, curved like the letter S. Here is a specimen. During the distillation the mixture of oil and water runs into this receiver, where they rapidly separate, as stated above. The oil floats on the top, and when the height of the mixed liquids rises to the upper opening of the S tube the water runs from this orifice, so that the level of the liquids in the receiver is always the same. The layer of oil increases from time to time, and may be drawn off from the water as necessary. The distillation ended, the mixture of oil and water is emptied into a glass funnel with a fine neck, which may be stopped with the finger till the liquids separate, and thus the water let off, leaving the oil alone in the funnel; or the oil is taken up by means of an instrument called 'pipette,' of which I also present a specimen. The oil is then bottled and hermetically sealed. This Florentine receiver is used only where the oils are lighter than water. Those that are heavier are received in cylindrical vases, whence the liquids are separated by use of the funnel, or by means of a tap.

"The following rules for the distillation of essential oils are given by De Fontenelle, a French chemist:—1st. To obtain quantity and quality, operate on a sufficient mass of plants. 2nd. Distil rapidly. 3rd. Cut in pieces the substances for the still, so as to facilitate the yield of the oil they embody. 4th. Employ no more water than is necessary to prevent the plant burning. 5th. For exotic substances, of which the oil is heavier than water, add salt to the contents of the still, whose increased density demands a higher temperature to produce boiling. 6th. For indigenous plants, redistil several times the same water on new substances of the same nature, thereby to more fully saturate it with the same oil. 7th. For oils naturally fluid often refresh the water of the worm; but keep it at 45° to 50° for the oils which concrete easily, as those of aniseed and roses. The quantity of oil annually yielded is relative to seasons more or less wet, more or less warm, the nature of soil, its exposure, and the maturity of the plants.

"The practice of distilling essential oils may receive further illustration by the following examples:—

"1. Thus, to extract the scent or essential oil of orange flowers, gather them when dry in early day after the dew is off and before the noon heat, remembering that those flowers just fully expanded contain the most oil. As soon as possible after the flowers have been picked place them in water, soak them in it for 24 hours, then distil. The proportion of water should be about a quart to every 2 lbs. weight of flower. The product will be oil, and water scented with the oil. The oil is called 'Neroli,' and is worth £28 the quart. The water should be distilled again on fresh orange flowers to produce more oil, and eventually also that useful tonic and antiseptic—orange flower water. The scent of 'Neroli,' as of the orange flower water, is that of the orange flower itself. But if you distil orange leaves they yield a scent of quite a different character, one of the lemon class, and called 'Petit grain.'

"2. Oil of Rose.—This oil, or otto of roses, is specially prepared in Turkey and Persia, by distillation, from the rose pâle (Rosa centifolia—cabbage rose), and the Damascus rose (Rosa semperflorens), which grow more odoriferous in those countries than in Western Europe, and hence the oils they yield have a special value. The otto is obtained by distilling many times the same water on rose leaves to which salt has been added. Thus, they take—say of rose leaves freshly gathered 60 lbs.; of common water 11 quarts; of salt 1 lb., and distil until no more volatile oil comes off. This oil is soluble in water, to which it communicates its odour, and constitutes rosewater, triple, double, or simple, according to the quantity of oil with which it is charged. In Europe the Rosa gallica, or Rose de Provins, is chiefly used. Note, in passing, that five kinds of roses appear to be cultivated for scents:—1. Rosa centifolia (cabbage or Provence rose), above 100 varieties (of which are our moss roses), from Caucasus; all cabbage-head, slender footstalk, drooping and graceful. 2. Rosa gallica (Provins rose, native to Europe), grown largely near Provins, a town in Seine-et-Marne (the druggists use this for making conserve of roses), above 100 varieties; a compact erect-growing plant, with large, open, flat flowers on stiff, upright stalks—the architectural rose. 3. Rosa damascena (Damascus or semperflorens) differs from centifolia in the large size of its prickles, the greenness of its bark, its elongated fruit, and long reflexed sepals, above 100 varieties. 4. Rosa moschata (Muscat rose), 10 or 12 kinds; slender branches, require support, never want pruning, flowers only at extremity of shoots. 5. Rosa sempervirens (evergreen rose); this rose furnishes the famous essence of rose of Tunis.

"3. Oil of Peppermint.—Made by distilling the plants in water, and redistilling that water in fresh plants. The mint is best taken at the moment of its flowering, and the soil chosen for its cultivation should be well exposed to the sun. Before distillation the mint is stripped of its branches, and left for a day infused in water. The oil of mint has a greenish colour, and is soluble in alcohol and in water. The first solution is called 'Essence of Mint,' and the second 'Mint Water,' of which great use is made in medicine as a cordial, vermifuge, &c.

"4. Oil of Lavender.—Lavandula spica (or aspic of Provence, Roussillon, Languedoc); and Lavandula vera (garden lavender).—Obtained by the distillation of the flowers as per practice with orange flowers, also by infusion of flowers in alcohol.

"The above are pure products of distillation, the extraction of unmixed scents—scents in themselves valuable, but specially so as forming the material out of which many delicate perfumes are subsequently prepared; for scents blended form new compounds—some harmoniously, and others to neutralize and destroy. Piesse classifies them as the notes of music, and in the best of perfumery the skilful blending of pure scents is a matter of interesting study. Thus eau de cologne owes its excellence, and as a commercial speculation has brought wealth to its manufacturers and their country, through a great success in the blending of scents. Three species of plants produce it, viz., the vine, the orange tribe, and rosemary. Piesse states that the first quality of eau de cologne is produced of the following ingredients:—Spirit, from grapes, 60° over proof, 6 galls.; otto of neroli, pétale, 3 ozs.; do. do. bigarade (Seville orange), 1 oz.; do. rosemary, 2 ozs.; do. orange zeste (peel), 5 ozs.; do. citron peel, 5 ozs.; do. bergamot, 2 ozs.; mixed with agitation, then allowed to stand a few days perfectly quiet before bottling.

"Of Distilled Waters.—Distilled or aromatic waters, as disseminated in commerce, are preparations composed of water having an essential oil held in solution. These waters, used in perfumery, in medicine, and in domestic economy, are prepared by means quite identical with those employed in obtaining essential oils, excepting that a greater quantity of water is used in their distillation, and that a more vigorous and lively fire can be used to give off in a stated time a greater quantity of watery vapour. The essential oil is thus prevented separating itself so readily from the water, which retains a greater proportion of it in solution. The waters of anis, peppermint, coriander, fennel, absinthe, thyme, &c., are prepared thus—by taking one part in weight of the plant and four parts of water, and distilling thence a produce equal to two parts only. The waters of rose, lime-tree (tilleul), &c., by taking one part of plants to two parts of water distilled to a produce of one part. The waters of orange flower, double, one part of flower to three of water distilled to two parts; or if only a half of this produce be obtained, viz., one part, it is then called orange-water quadruple. For Distilled Waters.—The rules laid down by Chevalier are—1. If the substance to be distilled has a close texture, or contains little water of vegetation, it should be broken up small, grated, or divided into morsels, and left some time in water, to allow the vegetable fibres to become well saturated, and so facilitate the exit of the volatile principles. 2. If the plant is comparatively inodorous, redistil the product of the first distillation on fresh plants as often as desired. 3. If the plant be highly odorous, put enough in the still to saturate the water sufficiently at one distillation. 4. Take care that enough of water is in the still to cover the plants till the distillation is over. The more the plant is succulent the less it needs of water. 5. Where it is apprehended the plants will soften by their boiling, so as to form a paste at the bottom of the still, support them by means of a wicker false bottom, or a metal diaphragm, or a bed of straw. 6. Bring the water rapidly to the boil, and sustain the boiling to the end. 7. Cool the worm as often as possible. 8. Use fresh plants in preference to dry, excepting balm, which gains odour in drying. 9. Filter the aromatic waters after their distillation, so as to separate any drops of volatile oil which may often be in suspension, and render the waters dangerous for use. Immediately after distillation these aromatic waters have a taste of the acid and disagreeable, which passes off in a little time; also often, after some days, flakes of mucilage are formed, which rest in suspension in the fluid or are precipitated, and give a disagreeable taste to it. If this happen the waters should be redistilled, preserved in glass or crockery vases, and filtered to take off the mucilage. Never cover these vases with linen, but with paper, as under the linen a mouldy taste will arise. For example, to obtain—

"Peppermint Water.—Take clean fresh leaves and flowers of peppermint, one part; water, four parts; and after 24 hours of maceration (soaking) distil to a product of half the water employed. If it is desired to intensify the product, redistil on fresh plants. Should there be a great quantity of plants to distil, take out the mint already distilled with a skimmer, and add new plants to the liquid left there, which being in a boiling state will much shorten the second distillation. In a like manner are obtained the waters of wormwood (absinthe), hyssop, marjoram, balm, rue, sage, thyme, &c.

"Orange-Flower Water.—Bring the water in the still near to boiling point; place the flowers in, and stir well; put on the still-head and distil. If you run off 1,000 grammes of produce for each 1,000 grammes of flowers, the product is called 'orange-flower water double;' if 1,500 grammes for 1,000 grammes flowers, 'treble.' The distillers of Grasse prepare another kind of water of orange-flower with the flower-stalks and fresh leaves, to which they add 4 grammes of neroli for each 5½ kil. of water. Thus obtained, the water is more bitter and less fragrant, but is cordial, stomachique, and vermifuge. When these flowers cannot be procured fresh they are inported from Spain or Portugal preserved in salt, and yield thus a very fragrant water. Common recipe for orange-flower water—Take orange flowers fresh and clean, 12 lbs.; water, 40 quarts; salt, 1 lb. Put the water and salt in the still, light the fire, and when near to boiling point infuse the flowers, fix the head and worm, lute, and adapt the Florence receiver, and distil for 20 quarts of eau de fleur d'orange simple. The half, or 10 quarts, for double; the third for triple.

"Rose Water.—Take of fresh rose petals 18 lbs., water 50 lbs., and distil for 18 lbs. of water. Redistil on fresh roses if you wish a stronger product. Also, good rose water is made from salted rose leaves. To salt the roses, dissolve the salt in boiling water, and then infuse the rose leaves. They will preserve well for six months. Though they become brown, they will not yield less valuable water. Some pack the leaves in dry salt. In like manner are distilled the waters of acacia, lily, and other flowers.

"Citron Water.—Fresh peel of 80 citrons; water, 40 quarts; salt, ½ lb. Distil to 20 quarts. The waters of bergamot and of sweet and bitter oranges are obtained in the same way.

"Apricot Water.—Fresh apricots, 25 lbs.; water, 40 quarts. Distil slowly to 20 quarts. The same preparation applies to waters of prunes, quince, raspberries, and other fruits.

"Water of Marasquin.—Ripe cherries, 40 lbs.; raspberries, fresh and clean, 7 lbs.; leaves of wild cherry, 2½ lbs.; peach nuts, ½ lb.; orrisroot, in powder, 2½ lbs.; water, 40 quarts. Crush the fruit; soak in water 24 hours; distil with care to 20 quarts.

"But it has been stated above that essential oils dissolve in other oils and animal fats. These indeed have so strong an affinity for them that two other processes of their extraction originate in this fact. They are called 'maceration' and 'enfleurage.' Less dignified epithets, but clearer of perception, would be 'soaking' and 'conjunction.'

Flowers are put into liquid fat or into oil, and left till the fat or the oil has absorbed the scent; that is 'maceration.' Or flowers are spread over the expanded surface of oil or solid fat, and left till these substances have possessed themselves of the scent which had belonged to the flowers; that is 'enfleurage.' And by one of these simple processes some of the most exquisite of scents of flowers are procured. Scents of jasmine, heliotrope, violet, cassie, mignionette, tuberose, orange, and even the finest odour of rose, are prepared by one of these processes, or by both these processes; for, after fat has been saturated with scent through flowers soaking in it, the intensity of the saturation may be augmented by spreading over its extended surface, when cold, fresh flowers to yield it their additional perfume. For the maceration process the fat is kept liquid by means of the heat of water at a sufficient temperature surrounding the vessel in which the fat is placed. The French apparatus for this is called 'Bain Marie;' the English ideal of it is a 'gluepot,' in which indeed a mere experiment might safely be made. For the 'enfleurage' process I have successfully used a common dish, on which purified beef fat was spread to the depth of half an inch. The flowers applied were of jasmine. I present a specimen of the result in the shape of 'essence of jasmine.' The jasmine is a variety known as 'Spanish jasmine' (Jasminum grandiflorum). It grows well near Adelaide. I present a specimen both of the plant and of the apparatus for efleurage by means of either fat or oil, as used at Cannes. Hundreds of these are possessed by some perfumery establishments at Cannes and Grasse. These have been made by Mr. Dodgson, of Hindley-street. The oils used for the maceration of scented plants are neatsfoot and olive oil. The fats are mutton, hogs' lard, and beef fat. The last two are preferred. Both the oils and the fats should be perfectly sweet or inodorous before being used. Their preparation would give quite a distinct trade if the extraction of scents from flowers were carried on extensively and generally. For an experiment, take fresh fat, melt and strain it through a fine hair-sieve into cold spring water, having dissolved in it a little salt and alum. Repeat this operation three or four times. Then wash the fat five or six times in plain water, and finally remelt and cast it into a pan to free it from adhering water. The scented fats, alone or diluted, constitute the French pomades; or cut up small and soaked for 10 or 12 days in spirits of wine at high proof, and kept at a temperature of 75°, they give the extracts or essences of the perfumer; whilst the scented oils, expressed from the cloths on which they are gathered are the veritable 'Huiles antiques.'

"There is one process for extracting scents from plants yet to be named; it is by evaporation. The invention is recent, and by a French chemist (M. Millon). It consists in dissolving the essential oils in ether, and evaporating them over a gentle fire. The result is a buttery substance, embodying the primitive odour of the plant. This product is said to have the advantage of being absolutely unalterable by exposure to the air, and that it will keep for years in open bottles without losing its properties. If this judgment of it be true, it is fair to conclude that the importance of such a process must be great."

W. K. Thomas, Printer, Register and Observer Steam Printing Offices, Grenfell Street, Adelaide.

ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER READ ON TUESDAY EVENING, JANUARY 31, 1865, BY C. TODD, ESQ., F.R.A.S.

Subject—"The Comet."

"This Society will, no doubt, expect to receive from me some information respecting the magnificent comet which has adorned our sky during the last ten days. I have therefore drawn up a few remarks, which I now beg to present, at the same time apologizing for the extremely scanty information I am able to afford. It is at such times as these that one most regrets the want of powerful telescopes and a properly-equipped National Observatory—wants which I trust will ere long be supplied. Having been for some years an active practical astronomer at Cambridge and Greenwich, accustomed to the use of first-class instruments, it will be readily understood how much I sympathize with the regret expressed by Mr. Babbage that our Observatory should still be unfurnished with suitable astronomical telescopes. At the same time, while I agree with that gentleman in regretting that we are not in a position on such occasions as these to render any valuable aid to astronomical science, I must not omit to point out to you that at the time Mr. Babbage wrote I could scarcely have been able to supply fuller information than I did in my letter published in the *Register* on Monday, the 23rd, even had I possessed more efficient instruments, except, perhaps, some particulars as to the structural appearance of the comet, which a more powerful telescope might have revealed. I may also add that having the weight of a large commercial department resting upon me, and requiring my constant supervision, I feel no little hesitation in undertaking the direction of a National Observatory, unless I can have proper assistance. What I am desirous of obtaining for our Observatory are a large equatoreal and a good transit circle, similar to those mounted at the Melbourne Observatory. These would cost in England about £1,200, to which must be added freight, cost of suitable building, mounting, &c. To make profitable use of such instruments I should require at least one properly-qualified assistant. I feel that I have at present quite enough on my hands in the active management of the Telegraph Department and the supervision of the system of meteorological observations I have as yet but only partly organized. According to the Rev. Jas. Maughan's letter, published in the *Register* of the 25th, that gentleman first saw the tail of the comet as a white luminous streak on the horizon just after sunset on the evening of Wednesday, the 11th inst. (the rev. gentleman says 12th, so he may mean Thursday). If so, he was probably the first in the world who observed it. A few persons appear to have seen it on Tuesday evening, the 17th. It was observed at the Melbourne Observatory for the first time, I believe, on the evening of Thursday, the 19th, when its position was ascertained to be—right ascension, 20h. 38m.; declination, 35° 2′. It was observed again on the following evening, when its right ascension was found to be 20h. 45m., and declination 37° 10′, showing a daily motion in right ascension of 7 minutes, or 1° 45′ in arc, eastward, and 2° 8′ in declination to the south—*i.e.*, a general S.E. course. I was unfortunate in not seeing it till late on Friday evening, the 20th, just before the comet passed behind a bank of light cloud in the south-west horizon. The next night was unfavourable for seeing the comet well, dark clouds continually passing over and hiding it from view, but between the breaks it was extremely bright; indeed, appearing all the brighter and more splendid by contrast with the surrounding dark clouds. Sunday night was very clear, and in every respect favourable for examining the structural appearance of the comet. I had only a good Dollond's telescope having an object-glass of about 2½ inches aperture. But as seen through it, the nucleus appeared as a bright nebulous star—as bright as Alpha Gruis of the second magnitude, at the extreme end or head of the comet, having no surrounding anterior coma, and less bluff than on the preceding evening. The tail was exceedingly bright, and when the sky was quite dark was estimated to be at least 14° long, and slightly curved northward towards the star Alpha Gruis, a small star of the 5-6th magnitude (1752) in Indus being just on the northern edge. Subsequent measurements have made the length of the tail 14° or 13° 40′. I have taken a series of differential observations in azimuth and altitude, between the comet and Alpha Gruis, with an excellent 6½-inch theodolite, on several evenings since I first saw it, the result of which when calculated will be sent to the Royal Astronomical Society. Although involving much more labour in their reduction, the observations will necessarily be in every respect inferior to those made with the fine equatoreals at Melbourne, Sydney, and the Cape of Good Hope. Had, however, no other observations been taken elsewhere they would have been sufficient to deduce therefrom approximate elements of the orbit, and have served to identify the comet with any previous apparition. The accompanying sketch shows the approximate apparent path of the comet between the 19th and 30th January as given by my observations. Now the question naturally put by every intelligent person is—has this comet ever been seen before? Are you able to identify it with any previously-observed comet? Well, I must at once confess that I am unable at present to answer the question satisfactorily. No orbit has yet been calculated, and I can only hazard a conjecture of little actual worth. You are no doubt aware that comets are frequently observed through so small an arc of their orbit as to render it extremely difficult to assign any definite period, or to determine the dimensions of the ellipse; in fact, the observations are generally satisfied with a parabola and occasionally an hyperbola. Where such is the case we can only identify a comet with another previously recorded by similarity of orbits, as shown by what are technically called its elements. When, for instance, a parabolic orbit is deduced from the observations, we know—1st. Its perihelion distance from the sun; 2nd. The longitude of its perihelion; 3rd. The inclination to the ecliptic; and, 4th. The longitude of the node, which four quantities serve to fix the position of the orbit in space. Now, suppose a comet to be seen which, besides presenting somewhat similar physical characteristics, is found to move in nearly the same orbit as another comet previously seen, although the observations by themselves may be insufficient to determine its period, we shall be justified in regarding it as at least very probable that it is the same comet; and perhaps, by a closer analysis of the observations, &c., we may be able to prove it to be so beyond all doubt, especially if by searching the records of the past we find notices of still earlier apparitions. It was somewhat in this way that Halley identified the comets of 1531 and 1607 with that of 1682, since called Halley's comet, and justified his predicting its return in 1758, and that its perihelion passage would occur early in 1759. With improved mathematical analysis Clairaut, assisted by Lalande and Madam Lapaute, who laboured unremittingly at the calculations for six weary months, showed that its perihelion would be reached on the 11th April, 1759, or possibly sooner, owing to the action of Jupiter and Saturn, which together delayed it about 618 days. The comet was first seen in December, 1758, and passed its perihelion on March 13, 1759. At its last apparition it was discovered on August 5, 1835, within about a degree of the place predicted. Mr. Hind has since traced successive apparitions of the comet with more or less probability as far back as the year 11 B.C. The comet of 65, which passed its perihelion on August 5, and was described as the sword-shaped sign seen over Jerusalem at the commencement of the war which ended in the destruction of the Holy City by Titus, may have been the comet of Halley. Now, it has been suggested by Mr. Babbage that the present comet is identical with the magnificent comet of 1843, his suggestion being based on the remarks of Sir John Herschel in his "Outlines of Astronomy." It will be exceedingly interesting should it prove to be so, for it will set at rest the question as to the identity of the comets of 1668 and 1689, which Clausen considered were identical with that of 1843, having a period of 21 years and 10 months; while others maintained that the comet of 1668 was identical with that of 1702, and the same as that seen in 1843, with a period of about 35 years, as assumed by MM. Laugier and Mauvais, who noticed the comets of 1528, 1492, 1457, 1106, 1001, 685, 580, 369, 335, 194, 159, and B.C. 367, as possibly the same. The observations of the comet of 1843, although it was perhaps the finest ever seen, were not sufficiently accurate and extended to admit of more than an approximate orbit being calculated. The parabolic orbit of MM. Laugier and Mauvais best satisfy the observations in the opinion of the Arago. The comet was first seen at Conception, in South America, on February 27, when its distance from the sun was estimated at 5′, or one-sixth of the sun's diameter. It was then at its perihelion. It passed nearer to the sun than any previously known comet, and between the 27th and 28th of February it traversed 292° on its orbit. The length of the tail was 60,000,000 leagues, and the diameter of the head 38,000 leagues. It was not generally seen in Europe till the 17th March, though Mr. Cooper, of Markree, and others saw it in Italy on the 12th. At Greenwich the tail only was seen, and was at first noted as a cirrus cloud. In Australia you saw it in all its glory. Mr. Cooper was the first to suggest, in a letter to Professor Schumacher, its possible identity with the comets of 1668 and 1702; but it was only a guess. He was at Nice, and reports the tail as parallel to the equator, and under the line joining Eta Leparis with Gamma Eridani on the 13th March. He failed to see the nucleus that evening, it being too low down. The elements of its orbit, according to Laugier and Mauvais, are:—Perihelion passage, February 27, 34 G.M.T.; longitude of perihelion, 278° 17′ 33″; longitude of node, 357° 52′ 4″; inclination, 36° 20′ 33″; log. perihelion distance, 7·7793757; eccentricity, 0·99943383765; motion, retrograde. We will compare these with the elements of the comet of 1668. 1668—Perihelion passage February, 28.8 G.M.T.; longitude of perihelion, 277° 2′; longitude of node, 357° 17′; inclination, 35° 58′; log. perihelion distance, 7·68000; motion, retrograde. The foregoing elements were calculated by Henderson after the comet of 1843. He had previously deduced the following orbit, which differs widely; the observations were, however, very roughly made, and the places obtained by laying its track down on a map:—Perihelion passage February 24.78 G.M.T.; longitude of perihelion, 40° 9′; longitude of node, 193° 26′;

inclination, 27° 7′; log. perihelion distance, 9·39990; motion, direct. The former satisfy the observation better than the latter or first elements. The comet of 1668 bears a strong resemblance to the comet of 1843. It was seen only in southern countries, its tail, being seen at Bologna by Cassini, extended from Cetus to Eridanus, and subsequently to Lepus; but its head was lost in the rays of the sun. It was also seen at Tonquin by a French mission to Cochin-China; but the accounts do not agree very closely with Cassini's. As I mentioned before, Clausen considered the comets of 1668 and 1689 identical, and the same as 1843. The elements of the 1689 comet are as follows:—Perihelion passage, 1689, December 2·14, G.M.T.; longitude of perihelion, 271° 16′; longitude of node, 344° 18′; inclination, 30° 25′; log. perihelion distance, 8·01284; motion, retrograde. This comet was not seen in Europe, and the observations which have been handed down, being mainly extracts from a Dutch ship's log, are extremely rough, so that no great reliance can be placed on the foregoing elements. The tail only was visible at Pekin between the 11th and 15th December. The nucleus is described as having a daily motion of about 3°, and the tail 68° long in the form of a sabre. If the comet of 1689 is the same as 1668 and that of 1843, then it was just barely possible that the comet now visible might be a reapparition of that comet; but since writing the foregoing, I have read Mr. Ellery's letter to the *Argus*, giving approximate elements of the orbit of the present comet deduced from the Melbourne observations of January 20, 22, and 24, which settles the question. They can of course be considered as only a rough approximation, but are quite sufficient to show that the comet is not identical with that of 1843."

The following is the letter referred to:—

"To the Editor of the *Argus*.

"Sir—We have been able to observe the comet on most evenings since its first appearance here on the 19th instant.

"The approximate position I then gave was—Right ascension, 20h. 38m.; declination, 35° 2′ S.

"Although it has decreased considerably in brilliancy, it will have been observed that it has presented a more imposing appearance, from the great increase in the length of its tail, which at first only measured 7°, but now measures nearly 14°. The Assistant Astronomer has computed its orbit from the three following positions, viz.:—

January.				Right ascension			Declination.		
dys.	hrs.	min.	sec.	hrs.	min.	sec.	deg.	min.	sec.
20	8	18	6	20	45	44·8	37	7	3
22	8	27	58	21	0	3·7	49	27	57
24	8	50	1	21	13	48·9	43	10	44

"He finds that the comet is at present too near its perihelion, and the intervals between the above dates are not sufficiently large to admit of the orbit being accurately computed; they have been used, however, to approximate to it, whence it appears that its distance from us is only 3,530,000 miles, or about one-thirtieth of that of the sun, which is the probable reason of its improving appearance. The longitude of the node is 292° 6′, and that of the perihelion, 123° 11′; inclination, 5 6′; perihelion distance, 0·9473; and perihelion passage January 22; motion, direct.

"A comet with the above elements is not to be found in either of the catalogues we possess, so that it is probable it has not previously appeared to us. It further seems likely that it will be visible for some time, as its motion is nearly in the same direction with that of the earth.

"Although the comet was seen at this Observatory for the first time on the 19th instant, I have received communications from several gentlemen, showing that it was seen on the 17th and 18th instant at Deniliquin, Colac, Tarnagulla, &c. It was not seen by the astronomers at Adelaide or Sydney till after the 19th instant. Some of our later observations having involved comparisons with stars whose places are not well determined—which, from the paucity of Observatories in this hemisphere, is too often the case with stars in the southern heavens—it will be some months before they can be observed with our fixed instruments for that purpose. A considerable time must therefore elapse before the orbit can be computed with all desirable accuracy.

"I remain, yours faithfully,

"Robt. L. J. Ellery,

"Government Astronomer."

"Observatory, January 27."

W. K. Thomas, Printer, Register and Observer Steam Printing Offices, Grenfell Street, Adelaide.

ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER READ MAY 2, 1865, BY THE REV. J. E. TENISON WOODS, F.G.S., F.L.S., &c.

SUBJECT—"THE TERTIARY ROCKS OF SOUTH AUSTRALIA."

No. 1.—Introduction.

It is now about 65 years since attention was first called to the Tertiary Rocks of the southern portion of the continent, and since that time very little has been done towards establishing the period to which they belong, or to fix their succession. It is known that we have in the various formations exposed on the banks of our rivers and sea beaches sections of extensive deposits; but whether they belong all to one vast province, representing one era in the world's former history, or whether they are divisible into all the various periods which the tertiary epoch includes in Europe, has never been satisfactorily explained. I propose to publish, with the assistance of the Philosophical Society, a series of papers on this subject, and I hope by placing the facts at our disposal in regular order to obtain something like a satisfactory conclusion. In order to do this, it will be necessary perhaps to give a slight sketch of what has been already done, and to notice recent investigation in tertiary formations near our continent, from which I think much valuable light may be thrown upon our investigations.

The first person to notice our Tertiary Rocks was Flinders, who, in sailing along the Great Australian Bight, saw the high cliffs of limestone which there form so remarkable an object on our shores. He imagined them to be portion of a coral reef, and described them as of a white calcareous base for two-thirds of their height, and then capped by a stratum of brown earth, in neither of which could stratification be observed, because of the great distance at which the observations were made. The lower part of the cliffs seemed to be corroded into caves and burrows, showing the stone to be of a very soft structure.

It is a matter of regret that we have no observations beyond those of Flinders of these very interesting rocks. That they are fossiliferous there can be no doubt, because Eyre was able to ascertain that fact in the course of his overland journey to King George's Sound; but no collections of the fossils have been made, and to this day we can only guess at their character.

The next person to call attention to our Tertiary Rocks was Sturt, who, in his journey down the Murray, passed by and examined the celebrated fossiliferous cliffs of that stream. He collected a number of specimens, and they are figured and described in the account of his first and second expedition (London: Boone, 1843.) With the peculiar views held by Sturt as to the origin of these beds we have just now nothing to do. The fossils he believed he had identified, were either of Middle Eocene or Miocene character, but it may be doubted whether a single one of his identifications is correct. Only two works appear to be referred to, and these seem to me to be Goldfuss's "Petrefacta Germaniæ, Iconibus Descriptionibusque Illustrata" (Dusseldorf, 1826-40); and G. P. Deshayes' "Description des Coquilles Fossiles des Environs de Paris" (Paris, 1824-37). Unaccustomed as Sturt was to the investigation of those finer shades of difference which divide species from species, it is not astonishing that he was misled by appearances; for it has been remarked by our most distinguished Australian palæontologist, Professor McCoy, that the distinctions between some of the fossils of our Miocene and those of Germany, and some of our Eocene and those of England, are of the faintest possible description. In some of his definitions he was also doubtless led astray by the imperfect character of the drawings he had to consult. As it is of consequence in the investigation upon which we are entering to point out in what respect Sturt has been in error, I shall here enumerate those species which are figured in his work, and which are wrongly determined.

In plate III, fig 1, *Eschara celleporacea* is *Cellepora Gambierensis*, Busk, not determined until many years after Sturt's publication. Fig 5. *Cellepora escharoides* is a new species which I have named *C. lunularia*, distinct from *C. nummularia*, as I shall show in a subsequent paper. Fig 6. *Retepora disticha* is a species of *Idmonea*, which is not figured sufficiently in detail to enable one to determine. Fig 7. *Retepora vibicata* is a new species *R. Gambierensis*, to be described subsequently. *Glauconome rhombifera* is *Salicornaria sinuosa*, Hassall. Fig 2. *Eschara piriformis* is as well as I can make out *Eschara Hamiltonensis*—a new species which I shall have occasion to describe. Fig 4. *Cellipora echinata* is *Pustulipora rugosa;* new species. Fig. 9. *Scutella* is, according to Dr. Duncan, identical with an urchin found in the Miocene beds of Malta. Fig. 10. *Spatangus Hoffmannii* is *Hemiptagus Forbesii* (Woods and Duncan). Sturt thought that the species was identical with one found in Westphalia, and named as above by Goldfuss. Its difference was seen by me, and I applied a name given previously to it, I believe, by some members of the Geological Survey. Desor, a French palæontologist, has made a subdivision of the *Spatangi* family, and our specimen was removed into the *Hemiptagi* division by Dr. Duncan, who, in "The Annals of Natural History for September, 1864," published a description of the fossil for the first time. The other urchin figured by Sturt is evidently a *Cidaris*. Indeterminable.

The names of five other species are given belonging either to the Middle Eocene of the Paris basin or the Lower Eocene of the London clay. They are *Corbula Gallica, Corbis lamellosa, Cytherea lævigata, C. obliqua,* and *Ostrea elongata;* but as none of them are figured it is impossible to say on what basis the identification rests. There is a *Corbis* in my collection which on the surface of the shell bears a good deal of resemblance to the species just named, but the hinge is very different. It came from the tertiary beds of Hamilton, which contain many specimens in common with those of the Murray. The only living specimens exist in the China seas, and might be found on the north coast of Australia. Fig. 12 is *Pecten Gambierensis*, named, however, by Sturt, *P. coarctatus*, and under this title it appears in my work. The other specimens of Sturt's plate are casts and cannot be determined, with the exception of a *Terebratula*, to be noticed subsequently.

In justice to the palæontological researches of Sturt, it should be said that in the case of the *Bryozoa*, he is not to blame for the difference between the names he gave his species and those which they have subsequently obtained. The whole of that portion of science has been recently in a shifting and unsatisfactory state, until Dr. Busk published his "Catalogue of the Polyzoa and the Monograph of the same Fossils in the Crag." As it is, we can say no more than that we are delivered from an enormous amount of most embarrassing synonyms.

From the time of Sturt we find no attempt made to describe our tertiary rocks until that of Jukes, who, in 1850, published a little book entitled "Physical Structure of Australia." It did not contain anything more than a notice of our rocks. The author seems to have visited some of the beds, but did not notice the character of the fossils any more than to state that they were tertiary.

Leichardt made some observations on the geology of Australia, but they were never published; and the few scattered notes of Strezelecki on the geology of South Australia (the south coast) contain only description of one fossil (*Terebratula compta,* Sowby), which should be a *Terebratella.*

In 1859 a notice was sent by Mr. Selwyn to the Geological Society on the classification of our tertiary rocks. Without any reference to fossils, or the evidence on which his summary rests, he merely gives the results of his observation, which are as follows:—The upper gold drift he classes as Post Pliocene, together with the raised beaches and estuary beds. The Newer Pliocene is the older gold drift, with the red marine rocks of Flemington. The Older Pliocene is the lowest of the gold drift, with the Upper Brighton marine beds and the lignite beds of the coast. The Miocene beds are those of Corio Bay, Cape Otway, Murray Basin, and Lower Brighton beds. All marine, he adds, with characteristic Miocene fossils. The Eocene beds are at Schnapper Point, Hamilton, blue clays, with selenite and characteristic Eocene fossils.

The Rev. W. B. Clarke, in a paper read before the Philosophical Society of New South Wales in November, 1861, gives the above division of the Tertiary as one in which he fully coincides; but in commenting on that portion which refers to the Miocene bed he gives an opinion which I think it better to quote. He says:—"Mr. Selwyn ranks the Murray Cliffs as Miocene also. Most of the English geologists have considered them Pliocene; but the late Professor Edward Forbes believed them to be much older; and Dr. Hochstetter thought them older still, judging from some fossils I gave him, and others which he saw in the Museum. As the genus *Spondylus* occurs among them, and that genus has twenty species which are cretaceous and only twenty tertiary, and as *Spondylus* is an Eocene genus in Europe, the Murray deposits may be partly older than Miocene. I must here mention a fact with which I have become acquainted—that within a few months wells have been opened on the Murray Flats, and that at a depth of 120 feet fish teeth, said to be *Carcharodon* and as large as those from Malta, have been found. Now, as *Carcharodon* is a cretaceous and Eocene as well as Pliocene genus, these deposits on the Murray may therefore be probably as old as has been just stated, especially as the shells occur in clay, and not, as in the Murray Cliffs, in yellow limestone." After this Mr. Clarke goes into the evidence for regarding the older gold drifts as Pliocene.

Professor McCoy, in his "Essay on the Ancient and Recent Natural History of Victoria" ("Prefatory Essays to the Victorian Exhibition Catalogue, Melbourne, 1861"), gives a chronology for our Tertiary Rocks, which differs very much from that of Mr. Clarke, and to some extent with that of Mr. Selwyn. He regards the Geelong beds as Lower Miocene, and the Schnapper Point beds as Upper Eocene. From the enumeration of the fossils, however, he would seem to include the Hamilton beds with Geelong, instead of with the Eocene of the east side of Port Phillip.

Last of all, my own work on "The Geology of South Australia" gives a much higher level to the Murray beds and those of Mount Gambier, making

them both Pliocene. Palæontological determinations are, however, carefully avoided, except in the case of the Bryozoa. The corrections in the fossils *Hemiptagus* and *Pecten* have been already alluded to; but it should be mentioned, also, that the figure of *Terebratula compta* belongs to another fossil, not *T. compta*, but probably a new species. The *Nautilus ziczac* appears to be a correct determination, and its presence in the beds at Mount Gambier, while it is also a common Upper Eocene form in the Isle of Wight, is somewhat enigmatical.

I have given above all that has been done, as far as I am aware, towards a description of the tertiary beds of South Australia, with the exception of a paper forwarded by me to the Royal Geological Society, which was embodied with my work, and need not be further alluded to here. My object now is to describe the different beds, as far as I am able, before going into detail as to their palæontological evidences.

The uppermost beds we meet with in South Australia are loose shifting calcareo-siliceous sands, entirely devoid of fossils, and sometimes highly ferruginous. I believe that they are extensively distributed throughout the whole continent; and, as they lie above all the other formations, I believe they are the most recent beds we have—marine beds, that is to say, and excepting the raised beaches and estuary beds. Under these lies a coarse granular calcareous sandstone, easily decomposed, and stretching out upon the coast into reefs of fantastic form. It is irregularly stratified, and lies in patches here and there, sometimes of considerable thickness—as, for instance, Portland Bay. It generally rests there upon an older trap rock.

Underneath this is an extremely brittle white limestone of such fine grain as to be almost like chalk. It contains abundance of fossils and layers of flint. The fossils are confined almost to *Pectenidæ*, *Echini*, and *Bryozoa*, and if any others are found they are only casts. The testa of the *Echini* are always altered into crystalline carbonate of lime, and are often broken. The casts are very white, and are formed of the finest marine mud. No fragments of the original shell can be discovered in them. Corals are never found, but sharks' teeth occasionally, and then not of recent species. The flints are full of fossils, but do not seem to be from fossil sponges. Some strata of the rock are very hard and cherty, and in these the *Pectenidæ* are more numerous; in other places the stone is soft and powdery, not even hardening upon exposure. The above formation is what I generally distinguish as the Mount Gambier limestone. It is capped at Portland Bay by a coarse shelly deposit, on which the fragments are all so much broken that species cannot be identified, excepting a few oyster-shells and some sharks' teeth, belonging to recent species. A small fragment of the same deposit is seen at Mount Gambier.

Of the Murray beds I can say no more than from the descriptions of others; but I have seen a good many fossils, and can state their differences. At the North-West Bend the shells also occur in casts, often in the form of Selenite. Sometimes a mass of *Turritellidæ* is found completely converted into selenite, and quite transparent. The formation seems to abound principally with the above fossil, as well as a large species of *Nautilus*, and a *Waldheimia* also of large size, and, though very different from the *Terebratula grandis*, of the English Pliocene, has still some points of resemblance. *Bryozoa* seem to be common, and corals occur occasionally The tops of the beds are often covered with loose oyster-shells

There can be no doubt that the Murray beds and those of Mount Gambier show remarkable points of difference, though the character of the stone and some of the fossils are similar. It has already been suggested by me that the difference is one of climate and geographical, and not more than we have a right to expect where the latitude of one place is nearly three degrees south of the other. Probably, however, there may be diversities greater than can be accounted for thus. If so, I should consider the Murray beds the more ancient of the two, for reasons which I shall give at length when treating of the chronological arrangement.

The fossil cliffs of the Murray disappear long before the mouth of the river is reached, though I believe there is no dip or any elevation. It would seem to me as if the cliffs are the remains of an extensive formation which covered equally a large superficial extent of the southern portion of the continent. We know that a very great area of our tertiary rocks have been denuded away; in fact, the loose character of the strata renders them so liable to decay that, as before remarked, we only meet with them under circumstances very favourable to their preservation. Yet the fossils do not disappear. In the Murray flats deposits of shells occur, but are generally only obtained by digging. They are decidedly on a lower horizon than the cliffs; and this fact, in a locality where there has been very little disturbance of level, is an important aid in the determination of the relative ages of the beds. The fossils found in the flats are very different from the Mount Gambier shells, but possess some forms in common with those of the cliffs and those of Geelong and Hamilton, to be mentioned subsequently. Altogether, I should consider them passage beds between the cliffs and the deposits, which I shall now refer to.

At Harrow, about 120 miles from the coast, there is a small deposit of shells in ironstone. It it on a little bank in a creek leading into the River Glenelg. The fossils are entirely confined to the surface, and are so broken and worn, so altered, in addition, by a copious infiltration of hydrated oxide of iron, that it is very difficult to make out the species. A long familiarity with the Miocene fossils of Victoria has enabled me to determine a great many of the forms, which I think belong to the Hamilton deposits now to be mentioned.

At the Muddy Creek, a small tributary of the Wannon River, about five miles south of Hamilton, there is a very extensive bed of fossiliferous clays lying under a bed of trap rock some 60 or 70 feet in thickness. The strata, no doubt, owe their preservation to the volcanic stone which covers them, which here, as indeed throughout the whole of the Wannon River, is developed in the form of Doleritic Lava. It is in consequence of an accidental fissure, down which the stream flows, that the beds are revealed; but there can be no doubt that they cover a much greater area of ground than the immediate neighbourhood of the creek, for their characteristic fossils have been brought up from the bottoms of wells sunk at a distance of eight or ten miles. The strata are sometimes loose and sandy, but generally compact and even, in places converted into a hard rock of carbonate of lime. They abound in fossils of every kind, showing an equal deep sea bottom, on to which shells were swept from neighbouring islands. The aspect of the shells, according to Professor McCoy, denotes a warmer climate, and in fact some fossils are found amongst them which still exist, but only in a much warmer sea—such, for instance, as the large *Pectunculus laticostatus*, Lamk., which is only found now in New Zealand—not, however, in a much warmer sea—the *Flabellum Candianum*, Edw. and Haime, which is only found in the Chinese seas—a *Corbis*, *Crassatella*, and *Harpa*, all of which are now confined to the tropics. It should be mentioned, however, that a more uniform type of life is found throughout the whole of the Miocene deposits—that is to say, shells seem to have had a much wider geographical range than they are known to have now, so that the argument for a warmer temperature is not quite convincing. Again, a good many shells which are found as fossils in Europe in the Miocene strata are found still living in the Chinese or Oriental seas; so that it proves rather a migration and change of habit since the Miocene times rather than a great change of climate. Yet, in spite of all this, most geologists think that there still remains evidence of a difference of climate in the Miocene period, and the beds under consideration seem certainly to favour that idea.

It has also been pointed out that the deposit is very much like what would have accumulated round a chain of rocky islands, not only in the nature of the beds, but in the character of the fauna. Many of the species of fossils are scarcely distinguishable from forms now common in the Philippine Islands. This island character proves two things—first, that the Hamilton beds were deposited under very different circumstances from the Mount Gambier beds, which are deep-sea deposits. Were there no other facts to prove the distinctness of the two this would be sufficient, for both deposits preserve their peculiar characters, and can be traced to within six miles of each other. The second thing is, that the destruction of these islands must form one of the remarkable facts in the geological history of the Australian portion of the earth's surface.

Below these beds Professor McCoy places the Upper Eocene deposits of Schnapper Point; but I have not visited them, and cannot describe them. From the fossils in my possession I can see that they are blue clays, which preserve the most delicate markings on the shells; but of about two dozen specimens, I can only count about three which I have not found in the muddy creek beds; neither have I found any *Foraminifera* nor *Bryozoa*, though I believe the latter are to be obtained.

The difference between the Schnapper Point beds and those of Hamilton is that the former is a dark-blue clay with mica and no *Foraminifera*, and the latter a light-brown ferruginous clay, containing many particles of iron oxide, and teeming with *Foraminifera*, especially *Amphistigina vulgaris*, D'Orb., whose coin-like discs are found in every pinch of the dust. The Murray beds, again, have fewer *Foraminifera*, and the shells are more frequently casts. The Mount Gambier deposits have very few shells at all and no corals, but the beds are made up of broken *Bryozoa*.

I have already indicated the succession of these various formations as I regard them, but I must add that I do not give it as a matter which is decided. The only way to settle their position will be by the determination of their geological chronology, and this can only be done after lengthened investigation. The rule adopted towards all Tertiary Rocks is to ascertain the percentage of existing species in any given bed; and according as it is more or less than others, so it is newer or more ancient. I think we are almost sufficiently acquainted with our coast fauna to be able to enter into such an investigation, but the material wanted is a sufficiently wide examination of the fossils. Tried by such a standard, however, we are not without difficulties in the very first application of the rule. The Mount Gambier rocks do not possess a single shell or urchin which is known to exist upon our shores at present. Now, I have strong reasons for believing that the beds in question are very modern, geologically speaking; but this is certainly a fact which would militate against my view were there nothing else to support it. But, as I said, the shells, &c., are so few in number that we must depend upon the *Bryozoa* for any results we may hope to obtain. Only a very few years ago it would have been hopeless to look for any results from such a source; but now so much has been done in their classification that identifications are more easy with them than in the case of some of the mollusca. What these results are I hope to point out in the course of these papers.

The Hamilton beds being very rich in shells give us a good number which still exist, and many are found common to them and all the other beds; but this should not excite astonishment. Even the Eocene or most ancient tertiary formation contains about 10 per cent. of existing species, and as they can be traced into all the other formations upwards, of course such fossils must be common to all. I am not aware that it has as yet been decided what percentage of peculiar species constitutes a separate formation; but I think we shall find in Australia that each has a sufficient number of peculiar forms to justify a separation. The rule would seem to be that differences which cannot be accounted for by climate and geographical situation should be held to indicate a different kind of marine life, and an epoch in geology.

In accordance with these rules, I would propose the following subdivision of our Tertiary beds which contain fossils:—

Newer Pliocene.—Tertiary beds near Adelaide, Government House quarry, &c.

Older Pliocene.—Mount Gambier, Portland, &c.

Upper Miocene.—Murray Cliffs.

Lower Miocene.—Murray Flats; Victorian representatives—Geelong, Cape Otway.*

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* In separating the Geelong and Cape Otway beds from those of Hamilton, I believe I am justified by the fossil remains, but in making the Schnapper Point beds Middle Eocene, I differ from Professor McCoy, who makes them Upper Eocene. My reason is this:—Mr. Selwyn makes the Hamilton beds and those of Schnapper Point of the same age, but though many fossils are common to both deposits, they are not contemporaneous. The Hamilton beds are, however, older than those of Geelong, and since Professor McCoy determines the latter to be Lower Miocene, the former must be Upper Eocene. But the Schnapper Point beds are, according to the learned Professor, a step below those of Geelong; therefore they are Middle Eocene—an

Upper Eocene (not known in South Australia). —Hamilton, Victoria.

Middle Eocene (not known in South Australia). —Schnapper Point, Port Phillip.

I have only made this subdivision provisionally, and with considerable hesitation. I am but slightly acquainted with the Adelaide tertiaries, but I think they contain a large proportion of existing species; and this is a department

where the members of the Institute can make useful investigation. Careful comparison of the fossils with good collections of our coast shells is all that is required, and this can be done without any attempt at classification. For my own part I only offer the above classification as a basis upon which to work. This paper is intended as an introduction to a series in which the Mount Gambier fossils will be carefully described. It is

only by making ourselves familiar with the fossil forms that we can hope to arrive at any sound conclusion as to the identity or separation of different beds which are found at great distances from each other.

In my next paper I propose giving figures and descriptions of all the *Pectenidæ* of the Mount Gambier beds.

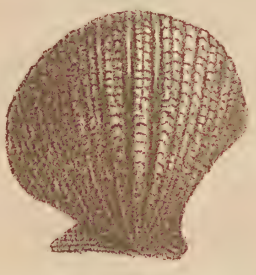
age which would tally with the remarkable resemblance shown between the fossils of the Point and those of Barton Cliff, Hampshire. The difference between the Professor's chronology and mine is in

my not regarding the Geelong and Cape Otway beds as identical; and in justice to the other side of the question I must admit that the corals hitherto described are Miocene in character, and

no characteristic Eocene form, such as *Turbinolia*, has yet been found at Hamilton, but this is also true of the Schnapper Point beds, of whose Eocene age there is no question.

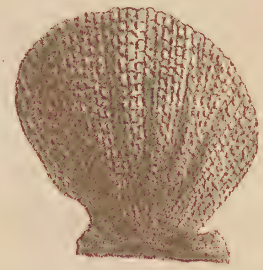
Andrews, Thomas, & Fisher, Printers, Register and Observer Offices, Grenfell Street, Adelaide.

Fig. 1.



PECTEN INCERTUS

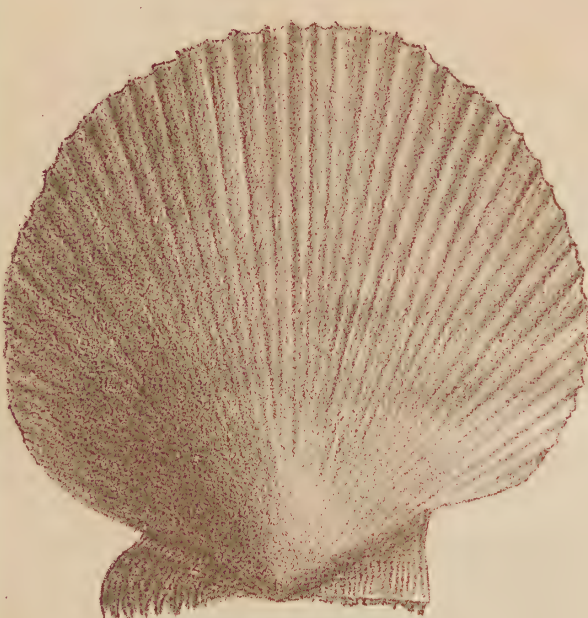
Fig. 2.



PECTEN GAMBIENSIS

Fig. 4.

B

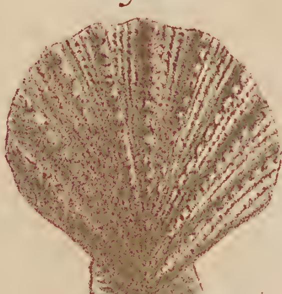


PECTEN YAHLIENSIS
LOWER VALVE

A

PECTEN YAHLIENSIS
UP. VALVE

Fig. 3



PECTEN FOULCHERI

Fig. 5

PECTEN PLEURONECTES

PENMAN & GALBRAITH, LITH.

ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER BY THE REV. J. E. TENISON WOODS, READ BEFORE THE MEMBERS OF THE PHILOSOPHICAL SOCIETY, MAY 2, 1865.

SUBJECT—"ON THE TERTIARY ROCKS OF SOUTH AUSTRALIA."

No. II.—THE MOUNT GAMBIER FOSSILS.

I have said already that the strata at Mount Gambier contain but few shells. The deposit was thrown down from a deep sea, and however much *that* is known to teem with life, far more indeed than any one suspected until late years, yet of mollusca, properly speaking, the numbers are very few. It will be as well to point out here the signification of this fact. That the sea which formed these rocks was a deep one we may conclude from the peculiar shells which are there found (*Terebratella*, *Waldheimia*, and sea urchins), as well as from the nature of the beds; but the absence of any other shells tells us plainly that not only was the sea deep, but it was very far from any land whatever. For though very few of the mollusca live at a great distance from the shore, because few can exist at a greater depth than about 100 fathoms, yet these shells are carried by the waters often to considerable distances, and to depths far greater than they would reach living. Thus it is that in deep-sea dredging shells are found in considerable number of species, which are not ever seen at any great distance from the land. Mount Gambier, therefore, and Portland Bay contain rocks which represent the bottom of a former open ocean where very little of the Australian Continent could have existed.

The small number of the entombed mollusca is at the very outset a barrier to any comparison between the shells of our coast and those of the deposits. The first shells which attract attention, both for their number and diversity, are the *Pectenidæ*. These are essentially deep-sea conchifera. Some are found at depths as small as 15 fathoms, but others range to 200 fathoms. They are especially numerous in the neighbourhood of coral reefs, but are found more or less in every locality and in every geographical situation. My impression is that the animals which lived in the shells found fossil at Mount Gambier have lived and died in the position where their remains lie entombed. Facilities for locomotion are not given to many of the mollusca; and though some *Pectens* can take surprising leaps, yet others are attached by a byssus, and in an allied genus — the *Hinnites*—by a calcareous incrustation on the lower valve. In a great many instances the valves of those found at the Mount are attached together, showing the tranquil state of the sea-bottom, and that the animal must have left his tenement in that position.

The *Pectenidæ* would include, according to the present classification, many genera which are not represented at Mount Gambier or Portland. The Pectens proper have been also subdivided into the following genera:—*Pecten*, free, regular, auriculated equivalve shell, with a transverse hinge, without teeth, but in place a perfectly triangular pit for the ligament; surface of the shell with longitudinal ribs, and often transverse scales; ears unequal—the posterior one with a sinus for the byssus. This genus has been subdivided into—

Chlamys—A sub-equivalve Pecten, with striæ in rays.

Dentipecten—Equivalve and obscurely marked; hinge teeth.

Pseudamussium—Vane-shaped, thin, smooth or delicately-striated Pectens.

Vola—Superior valve, flattened or hollowed.

Janira—Superior valve, much smaller, inferior valve curving round at the umbo to meet the hinge (all fossils and secondary).

Neithea—Hinge with teeth, and the general form leading by a transition to *Trigonia*. These are also secondary fossils.

Pleuroneitia. — Orbicular sub-equivalve, thin, gaping at each side; valves finely striated; ears nearly equal, the posterior presenting a sinus. This genus was proposed by Swainson as a subdivision in 1840, but it is not clearly distinct from the genus *Ammussium* of Klein.

Hinnites—With the lower valve adherent.

The above divisions include all the forms which we know as Pectens. Some of them only occur as secondary fossils, and therefore are not likely to fall in our way here. The other subdivisions, with the exception of *Hinnites*, I shall not adopt, because, 1st, the divisions are artificial and are not formed up on clearly-defined generic characters; 2nd, intermediate forms by which the characters of one genus insensibly approach another are constantly being discovered either living or fossil. Having premised this I now proceed to describe the Pectens found at Mount Gambier.

Genus Pecten, *Bruguières*, 1789. — Shell free, regular, inequivalve, suborbicular resting on the right valve, usually ornamented with radiating ribs; beaks approximate, eared; anterior ears most prominent; posterior side a little oblique; right valve most convex with a notch below the front ear; hinge margins straight, united by a narrow ligament; cartilage internal in a central pit; adductor impressions double, obscure; pedal impression only in the left valve or obsolete. (Woodward's Man., Moll., p. 257.)

Pecten maximus, Linné.—Two or three fragments of what I believe to be this species have been found by me at Mount Gambier. They are rather worn, and therefore the identification might not appear satisfactory to every one; but as *P. maximus* is a very variable shell, and I have compared it with some half dozen specimens in my collection, I make no doubt whatever that it is the same. *Pecten maximus* is a Pliocene fossil, but it still exists in the European seas, the fish being used as food. The occurrence of the species in Australia is singular, but I am not aware that it is found in any but the fossil state. It affords a strong proof of the Pliocene age of the beds, and perhaps a solitary instance of a species which is fossil here, and has living representatives in Europe. It is not at all uncommon to find species living in the Oriental seas which have not lived in European waters since about the Middle Tertiary times, so that, as I shall subsequently show, European Miocene fossils being found in Australian formations are no proof that the age of the beds is Miocene also; but a Pliocene and recent European species occurring as a fossil alone in Australia is curious, and offers strong ground for concluding that the strata in which it occurs are of Pliocene age.

Pecten incertus. Plate 1, fig. 1.—Shell solid, thick, globose, inequivalve, very inequilateral; ears unequal and strongly ribbed; ribs on shell about 30, bifurcating at their ends, alternating towards the edge with smaller ones all covered with granular imbrications. This shell resembles *P. asper* in some respects, but it is much more inequilateral and the imbrications more regular and square, though sometimes rising into a sort of spine. Length 1·5, breadth 1·5 of an inch. I have described the species because I believe it to be new, and because it strongly resembles a species found at Mount Gambier. The specimen, however, in my possession was not collected by me, and from its mineral appearance, and from the fact that I have never found the fossil in the Mount Gambier rocks, I very much doubt its occurrence there. It is, however, said to be common at the Murray beds and at Hamilton. The drawing represents a somewhat abnormal form.

Pecten Gambierensis, Plate 1, Fig. 2.—Shell somewhat thickened, corrugated, inequivalved, very inequilateral; ears unequal—anterior one rudimentary; posterior one large, rounded, and strongly ribbed, with a sinuation for a byssus. Shell with from five to seven broad corrugations, which, together with the intercostal spaces, are covered with small imbricated and granular closely-set ribs; granulations rounded. Lines of growth strongly marked in some specimens, making transverse ridges, from which the ribs proceed at a lower level and at a different angle, giving the fossil the appearance of deformity. Length 1·5, breadth 1·4 of an inch.

Observations.—This fossil very much resembles the last, but is distinguished from it by the ribs being more closely set and never bifurcating or presenting incostal ridges towards their termination. The granulations, also, are always rounded, approximate, and depressed, never subspinous. *P. Gambierensis* may be said to be the characteristic shell of the formation. There are few places in it where some specimens are not found; but it is especially abundant in the lower strata at Beswick's Cave, Mount Gambier, and on the walls of the caves at Mosquito Plains. It has in the disposition of the larger ribs a faint resemblance to *Pecten plica* of Linnæus.

Pecten Foulcheri. Plate 1, fig 3.—Shell thin, equivalve, inequilateral, with many fine radiating ribs and nine larger ones, on which are scattered coarse, unequal, laminated, or subspinous imbrications; length, 1·6 x 1·6 of an inch. Somewhat common at Mount Gambier, but nearly always found in a broken state.

Pecten Yahliensis. Plate 1, fig. 4.—Shell thin, equivalve, inequilateral; upper valve subconvex, lower valve much more so; upper valve covered with numerous regularly but faintly imbricated ribs; lower valve with about 30 well-marked but nearly smooth ribs; ears large and unequal, those of the lower valve sinnated with serrated fringe-like edges; upper ones ribbed and imbricated as on valve, with straight margin overlapped by lower; length 2·8, breadth 3 inches. The only perfect specimen of this beautiful species was got for me in sinking a well close to Yahl, Mount Gambier; but imperfect valves are not uncommon. The fringe-like serration of the ears give the shell a marked character, which distinguishes it from any other. It is certainly, without exception, the most beautiful fossil in the formation.

Pecten pleuronectes, Gmelin, *Pleuronectes Japonica* of some authors.—This fossil I have only found in two places, but I make no doubt that it is identical with the species now living in the North Australian seas. The shell is smooth, and on both the specimens in my possession there are most distinct traces of the rich reddish-brown colouring which is found on the upper valve of the living shell. *P. pleuronectes* is found as a fossil in the Upper Tertiary beds of the Department of Drome,

France, being one of a good many instances of fossils which lived in the Tertiary seas of Europe, but which are found living now only in Oriental seas.

General Observations.—The above forms a complete list of all the true Pectens which I have hitherto found in the Mount Gambier beds, and my search has now extended over a period of eight years. But though I have had facilities for the examination which could not fall to the lot of many, I am still of opinion that the beds have only been very slightly explored. Where the levels are so equal good sections are rare, and one has to depend entirely on caves or the sinking of wells for a knowledge of the fauna. Wells, again, do not always reach the beds; for in a large portion of the district water is found in the Post Pliocene shell beds, which, together with the sands of the Newer Pliocene, cover very large areas. From the list of *Pectens* given, I think, however, that the Older Pliocene facies is strongly marked. *Pecten maximus*, if a correct determination, is certainly not an evidence of any other than Pliocene age. I look, however, on the occurrence of *P. pleuronectes* as of more significance because of its being fossil in Europe and still existing in our tropical seas. For if it is granted that a migration eastwards of certain species of mollusca took place from Europe at the close of the Miocene period (and this is an opinion which is held by most palæontologists), then we should expect to find Miocene forms of Europe in Pliocene beds here. This, of course, supposes a migration of the species, not a restriction to a particular district of one formerly of world-wide distribution, because in that case *P. pleuronectes* might be Miocene or Pliocene. But it is a migration, I believe, which is held, while a more uniform type of molluscan life seems to have prevailed.

My next paper will include some of the *Brachiopoda.*

Andrews, Thomas, & Fisher, Printers, Register and Observer Offices, Grenfell Street, Adelaide.

ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER READ SEPTEMBER 5TH, 1865, BY THE REV. J. E. TENISON WOODS, F.G.S. F.L.S., &c.

SUBJECT—"THE TERTIARY ROCKS OF SOUTH AUSTRALIA."

PART III.—BRACHIOPODA.

The Brachiopods are bivalve molluscs, having one shell placed on the back of the animal and the other in front. These shells are always unequal in size, but symmetrical, and the ventral or larger valve is distinguished by a perforation somewhere near its summit, through which a ligament is passed for the attachment of the animal. The valves are articulated by two curved teeth, so completely interlocked that they cannot be even opened to a moderate width without breaking the shell. As I am now engaged upon an account of fossils I shall not delay with any reference to the anatomy of the animals, which have been fully treated in the works here referred to. The best treatise on the Brachiopoda is to be found in Davidson's Monograph in the Palæontographical Society's publication for 1853. The introduction contains memoirs from Professors Owen, Carpenter, &c., and while giving the most recent observations is completely exhaustive of the subject. The best compendium is to be found in Woodward's Manual of the Mollusca. The latter is amply sufficient for all ordinary purposes of reference. It will be necessary to mention that the name Brachiopoda was proposed by Cuvier, in 1805 (Ann. du Mus., vol. 1, p. 44), from the two variously-curved and cirrated arms or labial appendages with which each animal is provided. These arms not only create currents to bring them food, but also pass fresh supplies of oxygen over the mantle, which, in the absence of any special breathing apparatus, performs the office of respiration. Owing to this one anatomical peculiarity these shells are distinguished from all others by the possession of an internal skeleton, on which the arms are supported. The various ways in which this skeleton is modified in different species has formed lately the basis on which their classification is established; and as to this is due the structure of the animal, we possess more knowledge of the animal than of any other class of mollusca, and it follows that the classification is more natural and more strictly in accordance with sound zoological principles. Fortunately, too, the work has been taken in hand by the most eminent palæontologists we have, and numbers amongst its labourers the names of Cuvier, Dumeril, Von Buch, D'Orbigny, De Verneuil, Gray, McCoy, and Davidson. It is to the latter, however, we owe a complete restoration of the nomenclature and classification; for, unfortunately, the number of investigators increased the number of the synonyms most terribly, and it frequently happened not only that one specimen received three or four different names from different naturalists, but that even specimens of the same fossil would receive three or four different names from the same observer. To show, however, how enormous is the work in this matter, even though the investigations refer to only one class of a sub-kingdom, it may be mentioned that Mr. Davidson has been obliged to extend his observation to more than one series of papers in different publications, and n t only are the splendid monographs of the Palæontographical Society insufficient for the purpose, but even the numerous and lengthy papers in the *Geological Society's Journal*, the *Geologist* and the *Geological Magazine* leave the work still incomplete.

The Brachiopoda are, according to the author just named, the most elegant in their shapes and the most abundantly distributed of any in the Molluscan sub-kingdom. They are found in the oldest deposits at present known to contain vestiges of animal life, and have continued to exist, some in similar and many under different shapes to the existing forms. Their value to the geologist is consequently very great, because they appear in strata which contain no other fossils, and they possess in each geological period marked peculiarities of form, which enable the palæontologist to determine the age of any given deposit by their aid alone. All the various changes of form are more or less connected with the loop or internal skeleton for the brachial appendages, and it is perfectly astonishing how many varieties it has undergone. To describe the different modes in which the typical skeleton is represented would be a very lengthy task; but its magnitude may be estimated from the fact that about 43 genera and sub-genera are known, which between them number perhaps 1,000 species. But not only in *time* do the Brachiopoda enjoy a great range, but also in *climate*. They are found in tropical and p lar seas, in pools left by the retiring tide, and at the greatest depths hitherto explored by the dredge. (Woodward, p. 209, *et seq.*) At present o ly 70 species are known as actually living; but many more may yet be found in deep water, because these localities are necessarily but little explored as yet, and the Brachiopoda are found more in deep water than anywhere else Seventy species is said to be a larger number than any known to live at one time in the secondary system; but the class seems to have obtained its maximum of development in the Devonian or third great geological epoch of the primary rocks, and consequently in the very early dawn of the earth's history. There appears clearly to have been a great development, and then, after a thinning out, a gradual increase of representative species up to the present state of the earth's surface—a fact which would seem to be less easily reconciled with the Darwinian theory than any other.

Another peculiarity of the Brachiopoda is that the shells are studded with small perforations, into which short tubes of the mantle are prolonged. This enables the microscopist to determine fragments of the shells when they occur, as they frequently do in the Tertiary formation under consideration, mixed up with dust of Foraminifera and portions of Bryozoa. Dr. Carpenter states that the shell of the Brachiopoda generally contains less animal matter than other bivalves, but that *Discina* and *Lingula* consist almost entirely of a horny animal substance. There is not in these shells, nor probably in any of the Brachiopoda, a distinction between the outer and inner layers of shell which occurs in other bivalves. The loop processes are always solid and destitute of perforations. Most of the shells of the class found in the Mount Gambier formation are semi-transparent, and fragments are completely so, showing their structure well under the microscope.

Though the Brachiopoda have representatives in every formation, from the oldest to the most recent, yet the peculiarities of the modern forms are well marked and distinct. With the exception of one or two genera, they are all provided with an internal skeleton in the form of a loop. Some of the very ancient genera have representatives in even Australian seas; but as far as my knowledge extends, the Mount Gambier and Portland beds do not contain any. All the species I shall describe have a well-developed loop, and this peculiarity extends to the exclusion of all those genera, such as true Terebratulæ, which have only a very small one. I now proceed to describe the species.

Family — TEREBRATULIDÆ. Llhroyd, 1696.—[*Ichnographia Lithophylarii Britannici.*]

Animal fixed to submarine bottoms by a muscular peduncle issuing from a perforation in the beak of the larger or ventral valve. This aperture is partly surrounded by a "deltidium" in one or two pieces; oval appendages entirely or partially supported by calcified processes which commonly assume the shape of a loop, variable in form and dimension, but always fixed to the smaller or dorsal valve; shell structure always punctated. Seven genera and five sub-genera, all based on well-defined modifications in the form and position of the calcified supports of the arms.

Genus—TEREBRATULA.

Shell oval, elongated, or transverse; extremely smooth plaited valves, more or less unequally convex; margin even or waved; hinge-line curved; beak short, terminated by a foramen variable in size and partly margined by a deltidium in one or two pieces; *loop short, confined to the posterior portion of the shell*, and not exceeding much more than a third of the length of the valve, simply attached to the hinge-plate, the two band-shaped lamellæ are soon united by a transverse lamella bent upwards in the middle. The cirrated arms are supported by the crura, and project considerably in front of the loop; *no internal septum* in the dorsal valve.

Obs.—Some palæontologists (King, Gray, &c., in opposition to D'Orbigny, Davidson, and others, who maintain that the difference is generic) seem disposed to consider the species with short loops are the types of the genus Terebratula, and propose to place into separate sections and sub-sections those forms in which the calcified supports before being reflected or turned back (*Waldheimia*) extend to near the margin, as well as those in which the crura unite in the form of a band behind the mouth of the animal (Terebratulina); but these subdivisions, if of little value, are very convenient in the arrangement of the species, which may be even distinguished by external characters. Thus, in those forms with long loops the *mesial septum may easily be traced* on the external surface of the smaller or dorsal valve, while none such occurs in the short-looped species. (Davidson, loc. cit. The italics are my own.)

As yet no true species of Terebratula have been found in the Mount Gambier or Portland deposits, and this is the more singular as they are not uncommon in the Lower Miocene formation of Hamilton. One or two well-known species occur in the older Pliocene of Europe, but they do not exist in the present period. In fact, there seems to have been a gradual extinction of these forms in the Tertiary period until there has been only one species left living at present. It is not an Australian shell, and therefore we need not be surprised at the absence of these shells from our older Pliocene beds. When I say *absence*, of course I do not mean to say that there are none; but as none have been found yet, they must be rare, and will thus

prove the gradual extinction of this peculiar Brachiopod.

Sub-Genus—WALDHEIMIA. King. 1849.—*(A Monograph of English Permian Fossils,* 1849.*)*

Shell variable in shape, more or less circular, sub-quadrate, transverse, or elongated, with both valves convex, or with the dorsal concave; margin straight or waved; surface smooth or plaited; beak truncated and perforated by a circular foramen of variable dimensions, partly completed by a deltidium in one or two pieces; loop long, in general exceeding two-thirds of the length of the valve, formed of slender, shelly, riband-shaped lamellæ, simply attached by the crura to the hinge-plate, and more or less folded back on itself. The cirrated arms are partially supported by this appendage, and united throughout by a membrane exactly as in the typical species of Terebratula. The valves articulate by means of teeth situated one on each side of the deltidium, supported by plates, and fitting into sockets in the dorsal valve, in the interior of which is a prominent cardinal process and hinge-plate, with four depressions, under which originates a central mesial septum extending more or less into the interior of the shell.—[Davidson, loc. cit., p. 64.]

Waldheimia grandis. New species. Plate II., Fig. 1.—A, ventral valve; B, dorsal valve, and foramen of ventral valve; C, deltidium. Shell, smooth, very thick, elongated, convex; ventral valve, sub-carinated margin with two obscure plaits in the older specimens; beak short, obtuse, obliquely truncated, with a large circular cup-shaped thickened foramen. Loop two-thirds as long as the dorsal valve, lamellæ slender, straight, reflection unknown, septum as long as loop, gradually tapering, crura thick, semicircular. Length 2·2 inches; breadth, 1·4 inches. Mount Gambier. Rather rare.

Obs.—This is a very variable shell, but one whose thickened shell preserves it pretty perfect. I am not certain that it is properly referred to the sub-genus Waldheimia, because I have only been able to dissect two specimens. In both these instances the fossil was so completely converted into selenite that I could only obtain a knowledge of the structure of the loop by grinding away the dorsal valve in small portions at a time. I could not ascertain if there was any attachment to the septum; but had there been any I think I could hardly have failed to perceive it. The lamellæ were very slender, nor was there towards their centre any thickening, such as would occur at a point of attachment. Some specimens which I have seen are strongly marked with lines of growth, while in others these are scarcely perceptible.

It is a singular confirmation to the general rule of parallel organisms in our Tertiary beds that we should find large Terebratulidæ in the older Pliocene formations. In the same geological horizon in Europe we find *Terebratula grandis,* which has many points of resemblance to the fossil under consideration. As far as external characters go, one shell might easily be mistaken for the other. They are both large smooth and massive shells with obtuse posterior margins and large foramina; but the shell of *T. grandis* is thin and brittle, while that of *W. grandis* is very thick, and the posterior beak is much shorter in the European than in the Australian fossil, and the foramen larger. I have seen fragments of large Brachiopoda from the Murray beds, but could not state positively that they were identical with the species under consideration, though they resembled it closely.

Waldheimia Crouchii, new species. Plate II., Fig. 2.—The only specimen of this fossil which I have seen was a broken dorsal valve. It had seven large plaits upon the anterior margin, but these were only peculiar to the conditions of the animal, for the earlier lines of growth showed no such sinuations. Mere folds of this nature would not be sufficient distinctions on which to erect a species, because nearly all the larger or older Brachiopoda in this formation seem to possess them. As, however, the valve seemed to me to be flatter and the septum longer and thicker in proportion, with very small crura for the attachment of the loop, I have named it provisionally after my esteemed friend and fellow-labourer in these deposits, G. Godwin Crouch, Esq. The specimen was found at Mount Gambier. Length, 2·3 inches; breadth, 2·2 inches.

Waldheimia imbricata, new species. Plate II., Fig. 3—A, ventral valve; B, dorsal valve. Shell elongated, convex, angular in front, with a broad depressed keel on the ventral valve; margin waved; surface strongly marked with imbricated laminated lines of growth, which become sinuous and plaited as they recede from the posterior margin; beak sharply curved, with an oblique cordiform foramen; deltidium ribbed transversely; loop lamellar; curve unknown. Septum half the length of the shell. Length, 1 inch; breadth, 0·7 of an inch. Locality, Mount Gambier. Not common.

Sub-genus, TEREBRATELLA. D'Orbigny, 1847. [*"Consideration Zool. et Geol. sur les Brachiopodes, Comptes Rendus de l'Academie des Sciences,* 1847." There is a dispute as to priority, according to Mr. Davidson, between our own Professor McCoy and D'Orbigny. Mr. D. gives it in favour of the latter; but as far as I can gather I do not think that Professor McCoy referred to the same genus.]

Shell elongated or transverse, variable in shape; both valves regularly and unequally convex, or interrupted by a longitudinal depression in the dorsal valve; beak truncated by an oblique foramen of a circular or oval form, and partly margined by a deltidium in two pieces, at times disunited above the umbo; beak ridges more or less defined, and in some cases having between them and the hinge-line a flat or concave cardinal area; external surface smooth and variously punctated; loop, lamellar, reflected, but instead of being free during the whole of its course, receives two horizontal lamellæ at about the middle of the shell, which serve as supports.

Hitherto we have been dealing with a genus which has the loop free and detached from the septum. We come now to another, the peculiarities of which are as above described. It is not so extensively represented in species, but those which we do find are the commonest fossils of the beds. They are found in almost every foot of the stone, both at Mount Gambier and at Portland, and from this circumstance become a very good test to determine the contemporaneity of any deposits which are similar in mineral characters.

Terebratella compta. Sowerby *(Phy. Des., New South Wales, by Count Strzelecki,* p. 297). Plate II., Fig. 4.—A, dorsal valve, with deltidium; B, ventral valve; C, side view of both valves; D, interior of dorsal valve, showing reflection of loop and horizontal attachment; E, side view of dorsal septum and loop; F, horizontal lamellæ.

This species was described by Sowerby as follows:—Shell, smooth, thin, trapeziform; lateral margins sub-incurved, anterior margin small obtuse; hinge area large, with a longitudinal depressed line at either side; ventral valve triangular, rounded slightly, truncated in front with a small median sinus; dorsal valve faintly keeled; ligumental foramen terminal small and round. To this description I may add that the dorsal valve is sub-cordiform and flat, and both valves distinctly marked with concentric lines of growth. Septum round and solid, lamellæ of loop widening to the point of attachment, becoming again contracted at the reflection, and then extending into an almost complete circle with a slight projection towards the hinge.

In classifying this shell attention was drawn by Mr. Sowerby to the faint keel on the dorsal valve. This is the septum seen through the shell as a darkened translucent line. The existence of such a mark is a sufficient indication that the fossil does not belong to the true Terebratulæ.

Terebratella Tenisoni, new species. Plate II., Fig. 5.—A, ventral valve; B, dorsal valve; C, side view of both valves; D, side view of dorsal valve, showing prolongation of the septum; E, front view of dorsal valve, showing septum and muscular impressions. Fig. 6.—Same magnified.

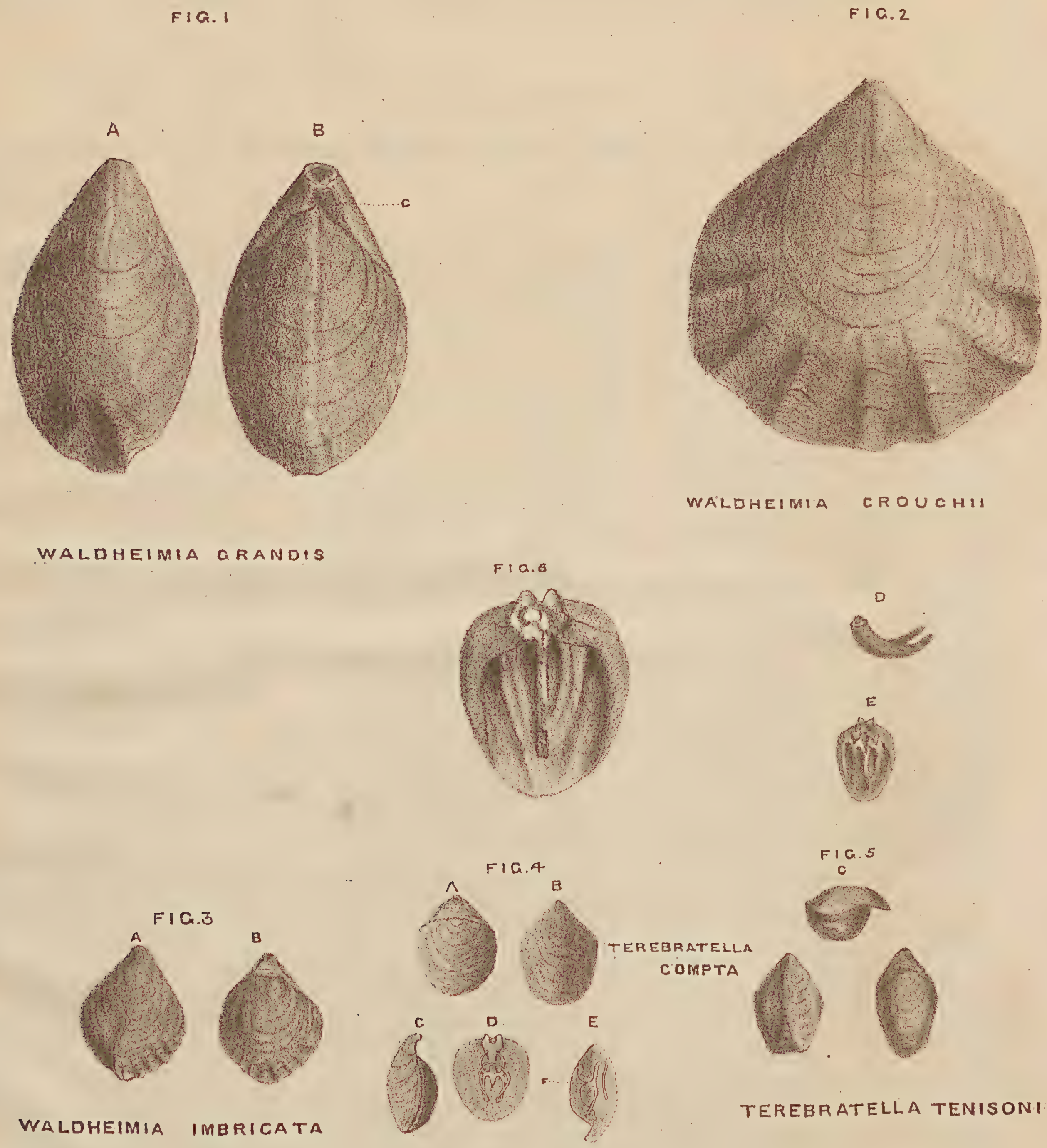
Shell elongated, unequally convex; ventral valve, trapeziform, keeled deeply, and terminating in a notch at the the anterior margin; dorsal valve orbicular, and tapering to a point which fits into the notch on the upper valve; beak obtuse; deltidium striated, trigonal; foramen oblong. Septum thickened, curved, and produced so as to touch the ventral valve; attachments of the loop at the centre and nearer to the shell than to the edge of the septum; muscular impressions deep, hinge and crura sloping away from the septum, with a deep sinus in the centre, giving it the appearance of a cervical vertebra. Locality, Mount Gambier and Portland. A smaller species, probably only a variety, found in Hamilton. Size, variable, but adult specimens 0·7, breadth 0·4 of an inch.

Obs.—This curious species has strong points of resemblance to the *T. Evansii,* found, I believe, in New Zealand, to which it is closely allied, but whose individual characters are very distinct. In both the septum is produced so as to touch the ventral valve. The anatomy of the muscular system must have been peculiar to cause such deep impressions, and I have no doubt that when this species is more carefully examined it will be found to possess features connecting the sub-genus with other more remote members of the family.

I cannot close this paper without expressing my obligations to Mr. J. R. Y. Goldstein, of the Land and Survey Office, Portland, through whose untiring assistance I have been able to ascertain the structure of the loops of the two species just described. It would be difficult to convey the care and nicety required in the examination. Fossils with perfect loops are not easily found, and in dissecting them the touch of a hair will shiver the lamellæ to pieces. They are the most fragile fossils I have ever seen. It is to the pencil of the promising young geologist whose name I have just mentioned that I owe the drawing of *T. compta,* and I believe he had to dissect 20 or 30 specimens before he succeeded in getting a knowledge of the structure. The loop of *T. Tenisoni* has not yet been seen.

General Observations.—The characters of the fossils I above described are decidedly recent, and though none of the species appear to exist at the present time, yet they are intimately connected with them. Thus the development of the mesial septum in *Terebratella Tenisoni* corresponds with the same peculiarity in *T. crenulata* as well as *T. Evansii,* but in no other member of the genus is there anything exceptional about the septum. *Waldheimia grandis* is already indicated somewhat similar to *Terebratula grandis* of the European Pliocene, but the fossil has been found in the Upper Miocene as well. Nothing can be concluded as to the climate from these fossils, as they lived in a very deep sea. Here, as in Europe, there is evidence of a tropical character in the fauna of our earlier Miocene formations; but whether or not this indicates similar conditions of climate is certainly open to question.

Andrews, Thomas, & Clark, Printers, Grenfell Street, Adelaide.



PENMAN & GALBRAITH

ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER READ JULY 11TH, 1865, BY HIS HONOR THE CHIEF JUSTICE.

Subject:—"The Science of Language."

The Science of Language is too extensive a subject to be satisfactorily treated in one paper, and I therefore propose upon the present occasion to confine myself to one branch only—that of the origin of language—and under this to enquire whether there are data from which we can draw any and what conclusions as to the source of languages and the manner in which they have been formed. That this is a perfectly legitimate subject of enquiry, falling within the range of scientific investigation, will, I think, at once appear from the consideration that all languages have been found to vary in ascertainable directions, and that there is a certain analogy between the modes of variation of different languages. The English of Queen Victoria differs sensibly from that of Queen Elizabeth, and yet more noticeably from that of Edward the Third. The French of Louis Napoleon differs from that of Henry the Fourth, and still more from that of Philip of Valois. The Greek of Plato differs from that of Homer. In the same manner every existing language spoken in countries which possess any literary monuments, going back only a few centuries, is found to differ from the earlier forms of the language which have been thus preserved. And it has been found by a comparison of these various changes that they have all operated in an analogous direction, so far as they affect the form of the language—in the direction, that is, of simplifying at once the pronunciation and the grammar. Letters and syllables are dropped out of words. Thus in English, levedi has become lady, timé time, Romé Rome, for originally both these words were pronounced as words of two syllables. Thus Gower has—

"So well I nought that any timé
Be lost of that thou hast do by me."

And

"To speke a goodly word unto me
For all the gold that is in Romé."

And at the same time the shades of meaning which were originally indicated by inflections of the original word, by affixes or suffixes, are either indicated by its position in the sentence or are left to be inferred by the hearer from his perception of the general meaning of the speaker, or are shown by the use of prepositions. To this process the term of phonetic decay has been given.*

Side by side with this process, however, there has gone on another, which consists either in the adoption of new words to express ideas which previously have been expressed imperfectly or not at all, or in a change in the meaning of old words. Thus, apart from the scientific words which are of almost daily coinage, in order to express new objects or new relations, the recent introduction and diffusion of the railway system in England has given rise to a great number of new terms or to new applications of old terms, some of which are gradually passing into common use. In the same way every new development of opinion or of feeling necessarily requires, in order that it may be adequately conveyed, either a new nomenclature or a change in the meaning of the old names if the old names are retained. And this change also appears to have rules to which it conforms. With regard to the coinage of new words, or their introduction from other languages to express new ideas, the reason of this is found sometimes in the picturesqueness or even the grotesqueness of the term, but more generally in the circumstance that it appropriately and completely represents some idea which could previously be expressed only imperfectly or by a periphrasis. Thus the word "taboo," introduced by Captain Cook from the Polynesian Islands, and which may be considered as naturalized, expresses the same idea as that of the Latin "sacer," or the Hebrew word which we translate holy; but as both of these words and their derivatives had themselves, so to speak, become "tabooed"—that is, set apart from ordinary use and appropriated to a peculiar class of ideas, and had, by reason of their habitual associations, become incapable of being applied without a revulsion of feeling to common objects—some word was wanted to supplement them, and this was found in the word "taboo." But, besides this conscious addition to our vocabulary, there is a constant process of change, by which new meanings are attached to old words, or new associations are connected with them, so that if retained they convey altogether different ideas, but often are altogether dropped out of ordinary use, and thus leave a vacancy, which some other word is required to fill.

Of the former, I may take as a familiar instance the word prevent, originally used indifferently in the sense of going before either to remove obstacles, or to place them in the way. In the former sense, which still survives in the theological term, "prevenient gracie," it is used in the Book of Common Prayer and in the Articles of the Church of England; but in ordinary use, the latter sense has come to be the only one attached to it. And with regard to the latter—that is, the case in which new associations become connected with words and lead to their disuse—any one familiar with the literature of the time of Elizabeth, and, still more, with that of Chaucer, will be able to remember enough instances to render any mention on my part quite superfluous. And my object at this moment is not to exhibit in detail the manner in which languages decay and are regenerated, but simply to call attention to the fact that these two processes, whose operations we can trace in all living languages at the present moment, have been in perpetual activity since the earliest literary records which we possess.

But if all living languages have thus derived their present form from general causes, which for the most part operate without any consciousness on the part of those who speak the languages, and if the same causes have been in operation in changing the form of languages which are no longer spoken, then we have obviously an instrument of investigation which we may apply for the purpose of ascertaining what were the earliest elements of speech; and, when this is approximately decided, we shall be in a position to form some opinion as to their origin and source. It need scarcely be said, however, that these conclusions must necessarily be in some degree conjectural. Up to a certain point we can proceed with confidence, because the materials exist to which our reasonings apply, and our conclusions therefrom are capable of verification. But when we have reached the limit of our researches in this direction—when we have determined approximately at least the various roots in a language—those elementary sounds which having been at first employed in some obvious, or at any rate in some definite sense, have been applied to designate a wide range of objects more or less connected by resemblance, analogy, or contrast; and when we seek from the data thus furnished to discover the source from which these roots themselves were derived, it must be confessed that our task is one of considerable difficulty, and the results must be regarded as in no small degree uncertain. And the best proof of this is the very different conclusions at which some of the most eminent men who have investigated the subject have arrived. But in this case, as in others, the differences of opinion are becoming less strongly marked in proportion as the subject is more thoroughly investigated and as the materials for forming an opinion are accumulated and sifted. And, at any rate, the subject has been so far elucidated as to permit a person, who, like myself, is only acquainted with the results of enquiry at second hand, to form his own conclusions upon the subject without being liable to the charge of presumption.

It has been a favourite idea with some writers, and probably may be said to have been the prevailing notion half a century ago—possibly in some quarters is so still—that language in the first instance was the result of a Divine revelation, and as a corollary from this that the original language was Hebrew. I need hardly say that such an idea derives no support from the Bible. In whatever light we may regard the narrative contained in the second chapter of Genesis, the idea that the writer obviously intended to convey is that the forming and affixing names to the various animals brought before him was the work of the man. He was to see the animal, and to utter some sound presumably corresponding to the idea which it excited in his mind. But there is no hint of supernatural assistance or suggestion, or of anything beside the natural play of the human faculties translating into sound, so to speak, the impression produced by external objects upon the new organization.

Independently, however, of these considerations, there are others which would seem to show that Hebrew, and in fact every language, has been not a revelation, but a growth. These considerations are derived from the circumstance that all the words in that language which express religious or moral or abstract ideas are derived analogically from words which have reference to sensible objects, and in a great degree to words connected with the parts or processes of the human body. Thus the Hebrew word for spirit, no less than the Greek and Latin words for the same idea, is derived from the word, or rather is the word which signifies breath or wind. Elohim is derived from a root which signifies strength. "In the Hebrew anger is expressed in numerous ways, all equally picturesque, and all alike derived from physiological facts. Sometimes the metaphor is taken from the rapid and animated breathing which accompanies passion, sometimes from heat or from boiling, sometimes from the action of breaking with a crash, sometimes from shaking; discouragement and despair are expressed by an inward dissolving, by the melting of the heart; fear, by loosening of the reins; pride is painted by elevation of the head, the form erect and unbending; patience is a lengthened, impatience a shortened breathing; desire is thirst, or paleness; pardon is expressed by a number of metaphors borrowed from the ideas of covering, concealing, passing over a fault, a cloth that wipes it out. To shake the head, to look at one another, to let fall the arms, are expressions which the Hebrew prefer to express the ideas of disdain, indecision, weariness, to any psychological expressions. Other ideas, more or less abstract, have received their name by a similar process. The idea of truth is

* Note—Max Müller, "Lectures on the Science of Language."

derived from that of solidity, or stability; that of beauty, from brightness; of good, from straightness; of evil, from turning aside, from crookedness, or from an offensive smell; to make or create is originally to cut out; to think is to speak; bone signifies substance." I have quoted this from Renan's work on the Semitic languages, not for the purpose of drawing any distinction in this respect between the Hebrew and other languages, but simply for the purpose of showing that the stage of the language in which we know it, and in which these various metaphorical expressions had become the recognised and intelligible means of expressing ideas not included in their original sense, and without which it could not have conveyed the lessons which it has been the means of teaching to the world, was the result of a gradual growth and development. The language must have existed for a long time before it reached the point at which it had arrived when we first became acquainted with it. And the people among whom it originated—those who first spoke it, could have had no ideas beyond those suggested by mere sensible phenomena, which alone their language directly expressed. That is to say, the literal must, in the order both of idea and of fact, have preceded the metaphorical or figurative use of words; and the latter could only have sprung up by degrees as new ideas were acquired, for which no corresponding words existed. These ideas, as they arose, were necessarily and naturally expressed by words, which either described their outward manifestations, as "loosening of the reins" for "fear," or described something which was felt to be analogous, as "straightness" for "justice."

But this suggests a feature common to all languages, namely, that the coinage, so to speak, of absolutely new words is one of the rarest of all phenomena. I have said that the introduction of new words—new, that is, to the language into which they are introduced—is constantly in operation; but none of these words, with few and inconsiderable exceptions, are really new—new, that is, in their elements. They are either introduced from a foreign language directly, together with the object which they signify, as "potato," or they are framed to express some relation or analogy of the object, or both, as "Pomme de terre." And even in scientific nomenclature, where names are systematically invented for new objects or new combinations, these are always derived from existing words, and are framed for the most part consciously upon the same principles as those which unconsciously influence the people in their selection. What has been said of Nature in reference to another branch of science—that of natural history—may be said of languages here: that they are prodigal of variety, but niggard of innovation. All that is required is effected by some variation in the form or some modification of the sense of existing words, and rarely or never by the making, in the absolute sense, of new sounds to express the new idea. The reason of this is obvious upon a moment's reflection. The first object of language is to convey ideas, and the necessary condition of this is that it shall be understood. Now a new idea is capable of being conveyed by a metaphor without any risk of its not being understood. To say of a just person that his conduct was straight, though obviously metaphorical, would be as intelligible, even the first time it was employed, as to say now to us that it was just. When the witness who was asked what he thought of some particular act replied that he should not say that it was quite the clean potato, no one for a moment would doubt what was the meaning he intended to convey. But if an absolutely new word is invented to express some new idea, then in order to its being understood it must at the same time be defined; and though this would be possible in a scientific essay, it would be quite out of place in the ordinary intercourse of mankind. In the case of a new object, the thing itself generally supplies the definition, as in the case of the potato referred to before; but instances of that nature do not affect the general rule. It may therefore not merely be taken as an established fact that in all languages the ordinary, if not the universal, process has been to express new ideas by modifications of the forms of existing words, or by new applications of their meaning; but we can see that such a process is the only one consistent with the object of language itself, viz., that of the intercommunication of ideas.

I have already said that the Hebrew language, in employing metaphorical expressions for the purpose of representing feelings, or relations, or ideas, only follows the course which all new languages have pursued. As Carlyle says:—"All unmetaphorical language you shall in vain seek for. Is not your very attention a stretching to?" And all these metaphors are originally derived from sensible objects. Thus, I quote from Farrar's work on the origin of language—"To take the first group of English words that present themselves, what is 'imagination' or 'reflection' but the summoning up of a picture before the inward eye? What is 'comprehension' but a grasping; 'disgust' but an unpleasant taste; 'insinuation' but a getting into the bosom of anything? 'Courage' is a good heart; 'rectitude' a perpendicular position; 'austerity' is dryness; 'superciliousness' a raising of the eyebrow; 'humility' is something cognate to the ground; 'fortune' is the falling of a lot; 'virtue' is that which becomes a man; 'humanity' is the proper characteristic of our race; 'courtesy' is borrowed from palaces; 'calamity' is the hurrying of the wind among the reeds. What are 'aversion' and 'inclination' but a turning away from and a bending towards? 'Error' is a wandering out of the way; 'envy' is looking upon another with an evil eye; an 'emotion' is a movement of the soul; 'influence' recalls the ripple circling on the surface of a stream; 'heaven' is the canopy raised over our heads; 'hell' is the hollow space beneath our feet; 'religion' [according as we may prefer the derivation of Cicero, or of Lactantius, or of Augustine] is a solemn study, or a binding, or a new choice; an 'angel' is a messenger; a 'spirit' but a breath of air; and we may carry our illustration into still higher regions—those of faith and worship. 'Mystery' is derived from 'mu,' the imitation of closing the lips; 'priest' from 'presbuteros,' elder; 'sacrament' is derived from the meaning 'oath'; 'baptism' is dipping; 'propitation' is bringing near; 'wisdom' is that which we have seen."

It is not necessary to show by analogous examples from other languages that the same rule applies to them. It may, I believe, be stated as a universal proposition that every word in all known languages which is employed to describe feeling and thought, or their processes or results, is derived from some word which originally expressed merely something cognizable by the senses. I will add only one further instance to those which I have already given, and that is the word which to us represents the highest of all ideas—the word God. The origin of our English word is uncertain, no satisfactory derivative having yet been suggested. But the words which in Greek and Latin and Sanscrit express this idea—Θεός, Deus, Dyaus—are all derived from a word which signifies the sky, and that word from a root which signifies brightness; and the same is the case, among other languages, with the Chinese. In Hebrew, as I have already pointed out, the word Elohim, which in the authorized version is translated God, is derived from a root that signifies strong or powerful. And Jehovah—or as the most eminent continental scholars consider that it ought to be written and pronounced "Yahveh"—is derived from the Hebrew word "to be," and that again apparently from a onomatopœia of breathing.

If, however, this is the case, then we are enabled to draw an important general inference, viz., that in their origin all languages expressed only the impressions which objects made upon the senses, and that everything else that we now find in them is the result of a development. This does not imply any opinion that the men by whom the language was first formed were incapable of perceiving the various relations, or of experiencing the different feelings which have been since embodied in language, but only that they had not arrived at such a stage of mental analysis as enabled them to separate the feeling from its manifestation, or to conceive of the relations as existing apart from the objects related. They may have possessed capacities equal to those of any of their descendants; but even if this had been the case they must, nevertheless, have passed through the process which every human being has now to pass through, viz., that of receiving ideas from impressions, and co-ordinating these ideas according to the nature of the impressions. It, however, justifies us in saying that if they had any innate ideas that were not derived from the senses, upon which I offer no opinion, they had no faculty which enabled them to invent an articulate sound that should serve as a sign for them so that they might be communicated from one to the other. And we can further say that in the process of co-ordinating their impression and of employing appropriate sounds which might represent them to others, they had not the assistance of the accumulated experience of prior generations; but had to work out the problem for themselves without assistance. The question, then, which we have to consider is—how was this done; what were the materials at their disposal; and what was the process by which those materials were rendered available?

The first difficulty which we encounter in entering upon this enquiry is that which arises from our utter ignorance of the primitive conditions of humanity. And we not only do not know, but it is scarcely possible by any effort of imagination to realize to ourselves any probable picture of their condition. In Europe we know, from recent discoveries, that at some unknown previous epoch there existed a race who left no other memorials of their presence than a few doubtful bones and some implements chipped out of flint and possibly some beads. But whether any of their descendants have survived or whether they have been all swept away by the races who intruded upon their country from the East is utterly unknown to us. In the East, to which we at least may look as the birthplace of our ancestors, there is nothing to show us an older condition of humanity than the shepherd's tent for the pastoral tribes, and cities and commerce for the settled agricultural communities; and both of these imply a long previous existence of man. To domesticate sheep and cattle—to learn the art of weaving, in order to make garments and tents—to have established the settled relations of master and servant, or owner and slave—all of these imply many generations, in which the foundation of such a state of society could have been laid, and many, in addition, during which these various occupations and relations could have become the normal state of the community. And agriculture and commerce require for their development a period of at least equal duration. In fact, with regard to the ancestors of the present civilized races of Europe and Western Asia, the language itself, whose origin we are now seeking to determine, furnishes us with the oldest glimpses of their early condition; but these, though they antedate all authentic history, carry us back only to a point very far removed from the origin of society or of humanity. We are, therefore, after all, compelled to have recourse to our imagination, in order to form a conception of the condition of mankind at the time when they formed their language, and to suggest the manner in which this work was accomplished; admitting fully, at the same time, that after all the picture which the imagination may frame can only be regarded as an approximation to the truth—depicting the manner in which language might have been formed, but not affording any absolutely certain grounds for the conclusion that in fact it was formed in that manner.

There have been two theories which have been proposed upon the subject—the onomatopœtic and the interjectional; or, as Professor Müller calls them with more conciseness than politeness, the bow-wow theory and the pooh-pooh theory—the former assuming that language is derived from the intentional or unconscious imitation of the sounds uttered by various animals, or produced by any phenomenon, as the cracking of a branch, the crash of a falling tree, the rattle of thunder, the rustling of leaves, and other analogous instances, for the purpose of describing them to others; and the other supposing that the object would as a rule be indicated by pointing it out, and that some sound would be used for the purpose of calling attention to it. To these now may be added a third, which—remembering the joke of Goethe, that if a German professor had to describe a camel he would retire to his study and proceed to evolve the idea from the depths of his own conciousness—one might be tempted to call the "professor" theory, that is, that language is self-evolved, that it is in some mysterious and inexplicable way the product of human consciousness; so that, to parody a famous expression, the man secretes language in the same manner as the liver secretes bile. At least this is the only sense I can put upon the somewhat ambiguous language in which this third theory has been put forth. It however appears to me that all of these theories, even the last, contain something of truth, and

that they are defective only in so far as they are exclusive; and of the three processes I should conceive that the first has been the most influential. At any rate, one thing would seem to be quite certain, that in the origination of language there are only two factors—man and external nature. All language must be the result of the effort to communicate to another, some impression which the speaker has received from some object, or some purpose or feeling in his own mind. Attending in the first place to the former, it is obvious that inasmuch as the means employed to communicate the impression is sound, it is most natural, to say the least, that the quality in the object which was selected as the mark by which it was to be recognised by the hearer should be some sound which it uttered or caused, whenever that was possible. Every writer on the subject at the present day disclaims the idea that there could have been any previous convention or agreement among men to determine the sense of words. And rightly; for to agree that any particular artificial sound should represent any particular object, it would be necessary that people should have a tolerably copious language to begin with. And yet without convention no sound could at first convey the idea of any particular absent object, unless it of itself recalled or suggested the impression which that object made; and that could scarcely be done unless the sound was in some degree imitative. *A priori*, therefore, it would seem that the bow-wow theory has at any rate the merit of accounting for a very considerable portion of the phenomena. And an examination of all languages shows that in fact, even now, after so many generations of speakers, when change of country, of circumstances, of habits, and even in some degree of organization, has tended to obliterate much of the early mechanism and sound of the words of which they are composed, each one of them bears deep and unmistakable traces of the operation of this principle of the imitation of sounds. And at the same time the interjectional element—the use of those natural exclamations which come to the lips, impulsively and instinctively, in any great sudden emotion of joy, or grief, or surprise—has also had its share in supplying sounds which helped to enlarge the infant vocabulary of the race. And I must confess that when we speak of the absolute origin of the vocabulary of language—the elements out of which it was originally formed—I am unable to conceive of any other than these two—the conscious employment of sounds which imitated or suggested natural sounds or objects, and the impulsive ejaculations which the momentary feelings of the individual might prompt, and which being the same for all would be universally understood. And when these are aided by mimetic gestures or actions, as is even now common with all uncivilized tribes, I think we might even see in these three elements means of intercommunication adequate to the few wants and simple relations of primeval humanity.

It is not perhaps easy to form an adequate conception of the extent to which these two vocal elements may be traced without the help of examples, and instead of attempting to choose those for myself, I have preferred to avail myself of the labours of others, and I therefore again quote from Mr. Farrar, who has, in the work to which I have already referred, systematized and abridged the works of other writers, to whose works I have no means of access:—

"As instances of the words which have arisen from the interjectional elements—*i.e.*, from the sounds whereby we express natural emotions—we may mention the large group of words that sprang from the root ach! ah! oh! as utterances of pain, as 'άχος, 'αχέω, *achen*, *ache*; or from the sounds of groaning, as vœ, wehe, woe, wail; or from an expression of disgust, as putere (Fr., *puer*), foul, fulsome; or from smacking the lips with pleasure, as γλύκυς, dulcis, geschmack, &c. This latter class is very widely extended, even in the Semitic languages. From the expression of disgust and fear we get awe, ugly, 'αγάομαι, 'αγασόμαι, and their cognates; from shuddering, the roots φρίσσω, bristle, hérisser, &c.; from the first sounds of infancy we get babe, bambino, babble, and many more; from the sounds of anger, huff and others; lastly, from prut, a sound of arrogance, we get the words proud, pride; as in German trotzig, haughty, from trotz, an interjection of defiance and contempt. The other class of onomatopœias is far more extensive, and embraces the widest possible range of inanimate sounds. They may be ranged under the following heads; and although the examples are all taken from the English language, they might be paralleled in almost any other:—1. Animal sounds, as quack, cackle, roar, neigh, whinny, bellow, mew, purr, cluck, caw, chatter, bark, yelp, &c. 2. Inarticulate human sounds, as laugh, cough, sob, sigh, moan, shriek, yawn, whoop, weep, &c. 3. Collision of hard bodies, represented by p, t, g, as clap, rap, tap, flap, slap, rat-tat, &c. 4. Collision of softer bodies, represented by b, d, g, as dab, dub, bob, thud, dub-a-dub, &c. 5. Motion through the air, &c., as whirr, burr, sough, &c. 6. Resonance, represented by m, n, &c., as clang, knell, ring, twang, clang, din, &c. 7. Motion of liquids, &c., represented by sibilants, as clash, splash, plash, dash, smash, &c. These are but specimens of the wide extent of these words in a language by no means the most remarkable for its adoption of onomatopœia. There are even broad general laws by which the various degrees of intensity in sound are expressed by the modification of vowels. Thus, high notes are represented by i, low broad sounds by a, and the change of a or o to i has the effect of diminution, as we see by comparing the words clap, clip, clank, clink, pock, peck, cat, kitten, foal, filly, tramp, trip, nob, nipple, &c. Another way of diminuting intensity is to soften a final letter, as in tug, tow, drag, draw, swagger, sway, stagger, stay, &c. Reduplication of syllables is a mode of expressing continuance, as in murmur, &c., and the effect is also produced by the addition of e and l, as in grab, grapple, wrest, wrestle, crack, crackle, dab, dabble, &c."

And it must be remembered that classes of words whose origin is thus indicated are necessarily but a portion of those which have similarly originated. They are only such as still bear the impress of the mint in which they were originally stamped after unknown ages of use. But there must be many others which have lost all traces of their first form, and many others whose legend, so to speak, we are unable to decipher because we do not understand the language in which it was written; or in other words, we cannot realize to our imaginations the feelings and impressions which prompted their original employment. For in this work of forming a language man was not a mere passive instrument returning one uniform response to the sights and sounds by which he was surrounded. The effect of these would be modified by the character of the race and the individual. The same object appears under very different aspects to men of different organization—to the man and to the woman, to the adult and to the child. The impression produced by an animal would not be the same to the bold man who pursues, and the timid man who flies. And besides this there are differences in sensibility to external objects. It is not merely in ages of refinement that to one man the primrose suggests thoughts of returning spring, and becomes an emblem of hope and promise; while to another—

> "A primrose on a river's brim
> A yellow primrose is to him,
> And it is nothing more."

But in all ages there have been some to whom every common object is full of suggestions, and these men would necessarily have the greatest influence in forming and enlarging the language. But then—of the almost innumerable aspects under which natural objects might present themselves to the fancy, according to the changeful mood of the observer, or the infinitely varying circumstances under which they might be perceived—who could hope that he should be able now to seize that one which was predominant at the moment, and which determined the sound by which such objects should for the future be designated. Of the 500 synonyms that are said to exist in Arabic for a lion, how are we to determine the one that represents the first vocables by which the idea of its anticipated presence was made known, or to explain the particular impression which these vocables were intended to represent? It is no subject of surprise, consequently, that in all languages there should exist many roots, original so far as we can determine, but for which we are unable to account upon either the imitative or the interjectional theory. But I think that this need not in any degree lessen our confidence in the conclusion that all the first representative sounds which formed the elements of language were derived from one or another of these sources.

It is obvious, however, that we have here only the elements of language—sounds adapted to communicate the existence of sensible objects, and, to a certain limited extent, of feelings; and to this extent there seems strong reason to believe some animals have language; at least they have the means of communication to this extent. A bee or an ant that finds a plate of sugar speedily makes its companions acquainted with the discovery; and the same is the case with dogs. Animals, too, have a great variety of sounds, which indicate pain, or pleasure, or alarm. The accounts given of monkeys proceeding on a plundering expedition appear to indicate almost an artificial code of signals, or at any rate signals by means of sounds, which to our organs are arbitrary, though they may have a natural significance to theirs. But this is not what we mean by language. And the question, therefore, is how a language could have been developed from these original sounds. And here comes in the third element—the human faculty. Starting necessarily from the same point as the animal—that is, from the sights and sounds of external nature—from impressions received through the senses, these impressions are in the case of man differently modified by his bodily organization and by his mental faculties. The action, so to speak, in the two cases is identical, or nearly so; but the reaction is different, partly no doubt because the impression received is to some extent modified by the difference between the organs of sense in animals and men, but principally because man possesses powers and faculties which, if not absolutely distinct in kind, are at any rate incomparably superior in degree to those possessed by animals. So far as we can judge, man alone possesses the power of indicating by language alone, the relations of time and place and person and number—to take these as among the simplest and most elementary of all ideas, and certainly to man alone belong the ideas of worship, reverence, trust, duty, authority, obedience, virtue, guide, shame. Not but that in some of these respects there is a vague sentiment in some animals answering to the feelings which these words represent, but that the feeling is always concrete, connected with a particular individual or act from which it is never dissociated, so that it never becomes an idea. And though there are some races of men who have no words corresponding to many of these ideas, yet there are none who have not some one or other of them, and none among whom some individuals may not be found capable of understanding most or all. And hence it is that though all languages bear unmistakable traces of having originated from the interaction of external objects and the human intellect, yet none of them have remained in their original condition. All, by various processes, analogous under many aspects, but differing as to their form and results in the different families of languages, have arrived at a condition in which they are able to represent by appropriate words such subjects and relations as the speaker, the person to whom he is speaking, and the person of whom he is speaking; the place where the speaker is, that which is near to that place, and that which is not near; the number, whether one or more than one; the relations of the family, father, mother, husband, wife, son, daughter; classes of objects, earth, water, tree, stone, bow, spear, dog, &c.; motion, going and coming, going forth and returning; feelings of joy, sorrow, pain, anger, &c. This is not intended as an exhaustive catalogue, but merely as an illustration of the various subjects which even the simplest known language is able to express; and it indicates two attributes of the human mind in its earliest stages by means of which imitative sounds are developed into a language—First, that intuitive or instinctive faculty of classification by virtue of which every word employed to designate an object becomes almost necessarily a representative of the class to which that object belongs; and, second, that perception of relations by virtue of which an idea suggests its opposite or its complement, as here, there; now, then; whole, part; good, bad; light, darkness; parent, child; me, not me, &c. Add to this the power of perceiving analogies between objects and ideas, between bodily sensations and mental emotions, and of expressing the one by means of signs originally appropriated to the other, and vocal organs capable of giving almost infinite modifications to a few simple sounds, and you have at once, as it seems to me, the origin of language, and the means by which it has been developed until it becomes an instrument of such compass, power, refinement, and utility as that English language which we employ.

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ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER READ AUGUST 15TH, 1865, BY THE REV. J. MAUGHAN.

SUBJECT—"THE DRAINAGE OF ADELAIDE, CONSIDERED IN ITS SCIENTIFIC ASPECTS."

"The subject which has to command our attention this evening is not only one of great practical importance, but one the consideration of which eminently accords with the objects for which this Society was founded. However important it may be to explore the various ramifications of natural phenomena, it is still more important to bring the teachings of science to bear upon questions of public utility. To deal amply and elaborately with things theoretical, and sparingly, or not at all, with those that are practical, would be as little accordant with the spirit of true philosophy as with the characteristic tendencies of the age in which we live. Amongst the many questions of great social and public importance which are commanding attention at the present time that of drainage is occupying the very foremost place. That imperfect drainage is the specific cause of many endemic diseases there cannot now be the slightest question. All medical and sanitary observation is in harmony with this fact. Wherever corrupting animal or vegetable refuse from human habitations becomes thrown upon the soil, or dejected into superficial cesspools, a twofold source of disease is immediately created. By the decomposition of solids, the atmosphere becomes charged with poisonous exhalations; and by the percolation of poisonous liquids, the water beneath the surface becomes a reservoir of death. There is then only needed certain peculiarities of constitution, or certain conditions of atmospheric moisture and temperature, to swell the bills of mortality, and prematurely send the inhabitants of a city to their long home. Thousands of cases might be cited, were it necessary, to show the intimate connection subsisting between sanitary science and the public health. In fact, it is now well known that typhoid or pythogenic fever—and which has sometimes been designated night-soil or cesspool fever—has in the great majority of cases its origin either in the percolations or in the exhalations of decomposing animal and vegetable matter. In an essay presented to the Royal Medical and Chirurgical Society in 1858, Dr. Murchison, who is one of the most eminent authorities on the subject of febrile diseases, gave a great many facts to show the relationship existing between this peculiar fever and the emanations from decomposing sewage matter. He stated that in 1829, at a school at Clapham, 20 out of 22 boys, within three hours, were seized with fever, vomiting, and purging. One of the boys died within 25 hours. The rest recovered. Suspicions were entertained that they had been poisoned; but after a most rigorous investigation the only cause for the illness that could be discovered was that a drain at the back of the house, which had been choked up for many years, had two days before been opened out, and its contents spread over a garden adjoining the playground. A most offensive effluvium escaped from the drain, and the boys had watched the workmen cleaning it out. One little boy, three years old, had been seized with similar symptoms on the day that the drain was opened, and died within 23 hours of the attack. In 1838, in the town of Birmingham, about 50 persons were seized with fever in the immediate neighbourhood of a small stream. This stream was little more than a common open sewer. The preceding season had been extremely hot, and the stream having become nearly stagnant, had given rise to odours during the night which were exceedingly fetid, and of which the inhabitants had greatly complained. There can be no doubt but that the fetid sewer was the cause of the disease. In the same year an epidemic fever desolated the Commune of Prades, in one of the departments of France. Of 750 inhabitants, 310 were attacked and 95 died. The cause of the epidemic was traced to a stagnant pool, which was the receptacle of dead animals and of all the sewage in the district. The outbreak was preceded by damp warm weather. Three times the pestilence returned, and always when the wind was blowing over the infected water. In 1846 a remarkable outbreak of fever occurred in a farmhouse in the thinly-peopled county of Peebles, in Scotland. Every one of the 15 residents was seized with fever, and three died. Many of the servants who worked at the farm were also affected. The only explanation of this outbreak that could be given was that the drains and sewers were found all closed up and obstructed with the accumulated filth proceeding from the privies and farmyard, and the effluvia from which were very offensive. In 1848 a formidable outbreak of fever occurred in the Westminster School and in the Abbey Cloisters, and for some days there was a perfect panic in the neighbourhood respecting what was called the 'Westminster fever.' Within little more than 11 days it affected 36 persons—all of the better class. In three instances it proved fatal. Shortly before its first appearance there occurred two or three days of peculiarly hot weather, and a disagreeable stench—so powerful as to induce nausea—was complained of in the houses in question. It was found that the disease followed very exactly in its course the line of a foul and neglected sewer, in which fæcal matter had been accumulating for years without any exit, and which communicated by direct openings with the drains of all the houses in which the fever had occurred. The Metropolitan Sanitary Commission gave it as their decided opinion that the epidemic arose from the bad state of the sewers and drains of the neighbourhood, but especially from the foul condition of the large sewer described. In 1852 a fever broke out at Croydon, which attracted great attention, and was made the subject of various official reports by the Board of Health. Out of a population of 16,000 persons 1,800 were attacked with fever, of which 60 died. The Commissioners appointed to enquire into the matter reported that an attempt had just been made to drain the town by means of stoneware pipes, which were insufficent in size, and which had been imperfectly cemented together; and that to the fetid emanations liberated during the opening and emptying of the cesspools, to the cleansing of old drains and open ditches, and to the foul gases which had been forced into the houses when obstructions had taken place in the narrow drain pipes, a great many of the cases of fever had been traced. In 1857, in the United States, President Buchanan and a large number of other persons who were staying at the National Hotel, in Washington, were all seized at the same time with enteric fever. It was reported that they had been poisoned; but after a most rigorous investigation it was found that the disease had arisen from the escape of sewer gases into the hotel. At one part of the building there was a direct opening into the sewer, and through this a strong current of fetid air was distinctly perceptible. The fever first appeared after three very warm days, during one of which the rain fell in torrents. The sudden rise of the River Potomac, into which the sewer opened, was supposed to have driven back the noxious vapours through the gully hole. In the same year typhoid or pythogenic fever broke out in Fleet-lane. Of 140 families scarcely one escaped. The disease appeared coincidently with the offensive smells that arose from the opening of a sewer in the lane. It was confined entirely to that neighbourhood. It continued during the whole of the four months that the sewer was kept open, and it disappeared as soon as the sewer was closed. In 1858 a terrible epidemic occurred at Windsor, and which was made the subject of special enquiry by the medical officer of the Privy Council. It was calculated that in four months 440 persons, or about one-twentieth of the whole population, were attacked with fever, of whom 39 died. The epidemic was traced to the gaseous emanations from the sewers. These sewers had been flushed with water from the Thames; but, owing to the long drought, the river had fallen greatly in its level, and the result was that the sewage accumulated in the sewers, and in consequence of their ventilation being very imperfect the sewer gases escaped directly into the houses. The fever attacked the rich and poor alike; but strange to say, no case of fever occurred at Windsor Castle, which had a special drain of its own, and which had been kept well flushed every morning by a plentiful supply of water; although in a portion of the Royal Mews, which was connected with the town drainage, and which was only separated by a roadway from the Castle drain, there were 30 cases of fever, of which three had a fatal termination. I wish I had been in a position to say that that terrible typhoid scourge had never been able to penetrate the sacred precincts of Windsor Castle; but the fact that we now owe allegiance to a widowed Queen confronts us, and the premature removal from our midst of that great and good Prince whom we had only begun to know at the moment that he fell bears testimony that Death's messenger can as easily climb the steps of a palace as he can lift the latch of a lowly door. That the death of Prince Albert was the direct result of inhaling a poisoned atmosphere it would perhaps be perilous to affirm; but from all the circumstances associated with that event, there is reason for more than fear that such was really the fact. After having dwelt at such length upon the poisonous exhalations arising from decomposing sewage matter, I need only dwell for one moment upon those percolations that are frequently found to poison our drinking water. There are two striking cases of this kind given in the essay to which I have already referred. In 1847 the inhabitants of 13 houses out of the 34 that composed Richmond-terrace, in Clifton, all drew their water from a well at one end of the crescent. The remaining houses were supplied with water from another source. At the end of the month of September it became evident from the taste and smell of the water obtained from the pump that it was tainted with sewage matter. Early in October, 'intestinal fever,' as it was called, broke out nearly at the same time in all the 13 houses where the tainted water had been drunk. In none of the other houses did it appear. The houses in which the fever broke out were far apart in the terrace, and there was little or no intercourse between their inhabitants. The water from the well was the only connecting link they had. In the town of Bedford, also, there was a severe outbreak of typhoid fever in 1859. On investigation it was found that the fever had arisen

from fæcal matter which had been permitted to soak into the wells from the numerous cesspools of the town. The water from these wells was found to contain a large quantity of decaying animal matter, evidently derived from the sources alluded to. There have been two theories propounded as to the actual nature of the pythogenic or typhoid poison. The first is the microscopic or fungoid theory, the other is the chemical or ammoniacal one. It is impossible, within the brief limits of a paper like the present, to give anything like a complete exposition of the fungoid theory. It may be stated, however, that millions upon millions of the subtle and impalpable seeds of fungi, as light almost as the particles of vapour that surround them, are continually floating in the air we breathe, swimming in the water we drink, and lying amidst the invisible dust amongst our feet. These germs of vegetation are only waiting the combination of a few simple circumstances in order to display their vital energies and to burst into free and independent life. Hundreds of millions of these invisible germs are perpetually floating about in the air-currents of our houses, and wherever animal or vegetable existence can be found. It is well known that many vegetable diseases are produced by the growth and development of these imperceptible fungi spores. The dry rot in our books, and ships, and houses is caused by the growth of fungi. The blights in wheat and in many others of the fruits of the earth, the mildews, the rusts, and smuts are all produced in the same way. There are also many animal diseases which are known to be produced by the development of fungi. In New Zealand a remarkable fungi group grows from the head of a species of caterpillar, which, instead of developing into a beautiful butterfly, becomes replaced by a nauseous fungus. In the West Indies wasps may often be seen flying about with fungoid plants nearly as long and as bulky as their own bodies growing upon them. Flies are often attacked by a fungoid disease at the beginning of winter when the cold damp weather which then prevails has reduced the vitality of their bodies to the lowest point and rendered them incapable of resisting external agencies. The silk-worm disease, called muscardine, has long been known to be occasioned by the growth of the fungi *Botrytis bassiana* within the body of the insect. Goldfishes are frequently destroyed by a fungoid growth upon their bodies, and even human beings become the soil in which they grow. The *Sarcina ventriculi*, which is most frequently found in the matters vomited by persons suffering under disorder of the stomach; the disease designated scald head, so frequently found on the heads of infants; the *Aptha*, or thrush, which often covers the mucus membrane of the mouth; and the *Sordes*, found on the teeth of persons suffering from low typhoid fever, are also so many different forms of these ubiquitous fungi growths. From these and many other facts the belief has long been entertained that the countless myriads of fungi spores that perpetually surround us are the advancing heralds of the plague and pestilence; that by their subtle poisons malarial and epidemic fevers are produced; that above us and around us, like vigilant spirits, they are perpetually roaming, watching to see that all is right within, but gladly availing themselves of the slightest flaw to accomplish our destruction. That there are many difficulties in the way of this fungoid theory it is impossible to deny. So numerous, indeed, are these difficulties, that it has been all but rejected by the great majority of scientific men. The other theory to which we have referred is that the typhoid poison is a compound of ammonia. Many years ago Dr. Winter expressed an opinion that the phenomena of typhus fever were occasioned by the presence of ammonia in the blood, and Dr. Richardson has shown that by the artificial introduction of sulphide of ammonium, one of the cesspool gases, into the blood, he has produced what may most unhesitatingly be considered as typhoid symptoms. It is well known that a pungent ammoniacal odour is given off by the skin and lungs in typhus fever, while a large quantity of ammonia in the breath admits of actual demonstration. Dr. Richardson found the breath of a patient in one case so ammoniacal that it actually coated with crystals of chloride of ammonium a glass slide, moistened with hydrochloric acid, and it also restored the blue colour to reddened litmus paper. There is not, however, such a wide discrepancy between the cryptogamic and this chemical theory as may at first sight appear; but whether one or the other, or both, may ultimately prove to be correct, it is perfectly clear that imperfect drainage is perilous to human existence, and that the inhabitants of a well-drained city will, on the whole, enjoy better health, and live to a more advanced period of life than those of one the drainage of which has been neglected. We must all then rejoice, as we all have a life-interest in this matter, that steps are being taken at the present moment by our City Council to preserve the public health, and to prevent the unnecessary waste of human life. There are some special reasons why the complete drainage of this city should be effected without delay. It is well known that the bed and the banks of the Thames, for nearly two miles on each side of the river, consists of a fine bed of gravel, and that upon this bed the original City of London once stood. Now, so long as the city was confined to the banks of the Thames, it had little need for artificial drainage. That bed of gravel acted as a great natural drain through which all its liquid and soluble sewage could percolate away. But no sooner did the inhabitants of London begin to build their houses on the impermeable clay beyond the gravel-beds than they found that their cesspools began to overflow, and their bills of mortality most fearfully to increase. A system of drains became immediately necessary; and happy would it have been for thousands had those drains been completed 50 years before they were. Unfortunately the capital of South Australia is not built upon a bed of sand. The marl and clay that lie immediately beneath our surface hold all our winter liquid sewage in suspension. The decomposing matters from our drains and water-closets mix with those superficial water-springs with which we are so familiar, and the consequence is that we are every day turning our city into what I have already designated a reservoir of disease and death. To help to remedy this state of things is the duty of every patriotic citizen. That it is remediable there cannot be the slightest question. Had the marshes of London been properly drained before the times of Oliver Cromwell and James the First, it is not likely that either of these two British rulers would have died of the ague. Had the City of London, with its narrow reeking steets, been burnt down before 1660, it is not likely that ever we should have heard the harrowing story of the great Plague. It is true that we have no reason to fear such a plague in this colony; but we have not yet forgotten the large amount of preventable mortality which took place in this city a couple of years ago; nor is it proper that we should forget it until all reasonable measures have been adopted to prevent its recurrence. Having said so much on the general necessity for good drainage, there are a few things that may be properly said on the methods which have been proposed amongst ourselves for the carrying out of this great work. As models of literary composition, little can be said of some of the essays which have recently been published under the direction of the City Council; and still less can be said of the artistic accuracy and general excellency of some of the illustrations by which they have been prefixed. The proposals, however, which they contain are of a somewhat varied character, and if these are not always in harmony with the teachings of sound philosophy, or with the results of matured experience, they at all events furnish starting points of discussion, and, in the majority of cases, bases on which a complete and efficient system of drainage may be built. The whole of the essayists agree as to the necessity for deep drainage, and surely no greater folly could be perpetrated than to waste the public money over such a partial system of drainage as would fail to drain the basements of the city. Such a system of drainage would, when the city becomes more densely populated, give satisfaction to no one, and if it were not ultimately superseded, it would certainly bring execrations upon its projectors which would be more expressive than polite. At the present time it is all but impossible to form cellars in many parts of the city. This arises from the causes which have been already named, and yet, as one of the essayists has properly pointed out, there is no part of the colony, in consequence of the great summer heat of the plains, where cellars are more needed than they are in Adelaide. Of the depth of the drains, the 13 feet, proposed by the third essayist, is decidedly preferable to either of the others. As to the capacity of the sewers and the mode of their construction, the writers differ in opinion, because they differ as to the amount of work required to be done. The first essayist proposes that all the ordinary rainfall shall be carried into the sewers, but that storm-waters shall be carried by overflow drains into the Torrens. The others both provide for storm-waters being carried away by the main drains. The necessity for either of these arrangements is not very manifest. To carry the surface water at all into the drains in a city where the rains fall only during four months in the year, and where, therefore, it is absolutely necessary to provide effective artificial means of flushing, appears to be unnecessary, but to carry the pure and innoxious storm-waters into the drains, only to turn them out again in a noxious and poisoned form, appears to be contrary to all principle of sound economy. As rain water runs quickly away, and is not injurious to health, there seems to be no necessity for taxing the sewer system for its removal. Wherever the gradients permit of its flowing into the Torrens, any one of the three watertables recommended by the third essayist would be sufficient for the purpose. There are two other subjects connected with this question of drainage which possess considerable scientific interest. One is the proper ventilation of the sewers, and the other is the treatment and disposition of the sewage. On the former of these, the first essayist has written with the greatest amplitude and clearness; but while either of the plans suggested would be suitable for a climate like that of England, neither of them can be properly considered so for a climate like our own. As the object of drainage is to get rid of the poisonous gases evolved during the act of sewage decomposition, it follows that next to the construction of the sewer stands the question of its ventilation. Where sewage decomposition is necessarily slow, rapid and perfect ventilation are not so imperiously demanded; but where the summer heat sometimes causes the decomposition of our butchers' meat within a few hours of its leaving the slaughterhouse, it follows that the greatest care will be required in the ventilation of our sewers. To open ventilators, then, in the middle of every street, at distances of only 40 yards apart, in a climate like this, is at least a doubtful way of getting rid of poisonous exhalations. Even to be dependent upon the fitful ventilation arising from the connection of the sewer with some casual steam-engine chimney would prove in the end to be questionable economy. It would be better to leave the money of the colony unexpended than to waste it for a doubtful benefit. The only really satisfactory method of ventilating the sewers of a city like this appears to be that proposed by Mr. William Hosking, the eminent London architect, and one of the Metropolitan Commissioners of Sewers. After speaking in strong terms of the absurdity of laying down sewers to get rid of poisonous exhalations, and of then turning those very exhalations out into the middle of the street, he says—"Now the course proper to be pursued is almost the reverse of that which is practised, and from which the inhabitants of sewered towns suffer in comfort and in health—more, perhaps, than the inhabitants of unsewered towns, in which there is commonly no such concentration of foulnesses. Give freedom to the foul gases of the drains, as freedom is given to the air which has served the purposes of combustion in the chimney-grate; make a flue for the foul air of the drain of every house, at or near to its upper end, as a flue is made for the escape of the burnt air of the coke or coal fire to pass off by, and both will alike rise into the upper air to be dissipated by the winds of heaven, and prepared by Nature's chemistry to reappear as the course of Nature prescribes. A lofty ornamental shaft, built at the head of every main line of sewer, and provided with the means of securing an updraught through the shaft—means to which the wind will always give effect in even the stillest weather—would give vent to all the emanations which arise in the sewer itself. But to assure this result there must be no trapping or flapping of the inlets to the sewer; the air must be allowed to pass down freely, which it will do—firstly, by its own gravity; and secondly, by the draught established in the sewer by the upper draughting shaft and by the house-drain flues." As the rectangular form of the streets of this city is eminently adapted for securing efficient ventila-

tion by means of Mr. Hosking's plan, I trust that in the interests of the public health the City Council will adopt the necessary means for giving it effect. On the final question of the treatment and disposition of the sewage there has been a considerable amount of new light thrown since the essays of the competitors were sent in. There can now be very little doubt but that both the solid and the liquid sewage may be turned by agriculturists, and therefore by the City Council, to profitable account. There is at the present time a great want of good manure for the lands of the colony. Many of the farmers with whom I have conversed tell me that, owing to the great length of the dry season of the year, they cannot profitably convert the straw raised upon their farms into vegetable manure, but that they would gladly purchase any suitable material to replenish their lands if it could be obtained within a reasonable distance and at a reasonable cost. With the railway, therefore, so close to the sewage outlet, as it is likely to be if any practicable scheme of drainage is adopted, there can be no doubt about finding a market for the deodorized sewage manure. And as to the outlet for, and the disposition of the liquid drainage, the new light that has been thrown upon its value when applied to such sandy soils as are found betwixt Adelaide and the sea is certain to make it of immense value to the owners of such lands. In the discussion which recently took place upon the subject of the London sewage before the Society of Arts, and which is recorded at great length in the journal of the Society for the 3rd and 10th of February last, it was stated that if liquid sewage were applied to undrained clay land, as in many of the recent experiments it had been, it would not repay the cost, but that if plentifully applied to gravelly soils it would make them abundantly productive. In proof of this statement, Lord Robert Montague said that he had recently visited some meadows near Edinburgh, where the sixth crop for that year had then been cut, and the seventh was then on the field knee deep. The land which had formerly let at 2s. 6d. was then bringing £40 an acre. If such results could be obtained in the cold climate of Scotland, what may we not expect to witness in the climate of South Australia? But I must now conclude my somewhat lengthy paper. There are several other points of importance which have been raised by the drainage essayists to which reference might be made were it not that they partake more of an engineering than of a scientific character. On the all-important question of cost, for example, I find that the second writer gives us no details of prices, but estimates the entire cost at £260,900. The third writer, with an average cost of 18s. per yard for sewers, makes the whole amount £102,768 10s., while the first writer — with an average cost of 27s. 6d. per yard, or 9s. 6d. more than the other—only makes the total cost £89,000. From this discrepancy it is evident either that the first writer must have greatly underestimated the quantity of work to be done, or that the third must have erred in the other way. As, however, I must not allow myself to be tempted into the discussion of the engineering part of the question, I will at once conclude by thanking you for your kind attention."

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ADELAIDE PHILOSOPHICAL SOCIETY.

PAPER ADDRESSED TO CHARLES TODD, Esq., F.R.A.S., VICE-PRESIDENT OF THE ADELAIDE PHILOSOPHICAL SOCIETY, BY G. W. EARL, Esq., AND READ BEFORE THE MEMBERS SEPT. 5, 1865.

SUBJECT:—"CONTRIBUTIONS TO THE METEOROLOGY OF NORTHERN AUSTRALIA."

To CHARLES TODD, Esq., F.R.A.S., Vice-President of the Adelaide Philosophical Society.

Dear Sir—A recollection of the many interesting conversations we held in Adelaide last year has prompted me to draw up a summary of my experiences in connection with the meteorology of the tropical region which includes your Northern Territory. I am sure you will peruse it with interest, and if it should also prove acceptable to the members of the Philosophical Society I shall feel proud to be a contributor to the annals of a Society that has done me the honour of electing me a corresponding member.

The trade-wind, which blows from N.E. in the northern and from S.E. in the southern hemisphere, is the ruling phenomenon in the meteorology of the entire tropical region which lies between the Indian and Pacific Oceans, as it is the source of the periodical winds or monsoons, and at the same time feeds or produces the rainfall, without which the lands included within this region would be utterly sterile and uninhabitable. It is only during the winter season of either hemisphere that the trade-wind blowing within its limits exerts a supreme influence. Thus, during the months of May, June, and July, the S.E. wind commences about the parallel of 28° S., clear and bracing, but as it approaches the equator the atmosphere becomes charged with moisture, and by the time it reaches the S. coast of Asia, where it is called the S.W. Monsoon, the load of moisture is so great that the downpour is often terrific. Again, the dry N.E. wind of the northern hemisphere attains its greatest strength in the months of December, January, and February, and as it approaches the equator becomes in like manner loaded with moisture. But there is no large tract of land in this direction to act like the continent of Asia as an attraction to the surcharged atmosphere—nothing in fact but open sea; with the trade-wind blowing over it from S.E. This obstruction has the effect of "packing" the rain-clouds in a belt of from 300 to 500 miles in width, which extends across the mouth of the Bay of Bengal and the southern part of the China Sea, moving gradually to the S.W. until it envelops Java and the eastern groups and eventually reaches Australia, where it sometimes penetrates even beyond the tropic. This is the westerly monsoon, for the surcharged atmosphere has the tendency that is observable in all rain-bearing clouds, when closely packed, of moving to the eastward, sometimes with a velocity that amounts to a gale.

West Monsoon, or Rainy Season.—This monsoon commences in October, in the Bay of Bengal, where the first spurt of N.E. wind blowing from the land stops the rain-clouds that have been flowing in from S.W. during the previous five months and turns them towards E. and S.E. This N.E. wind is the trade-wind, which continues to blow without intermission until the month of April, clearing the China Sea and the Bay of Bengal of rain-clouds, which by the end of the year will have been driven to the south of the equator, where the S.E. wind, which never ceases to blow in the southern part of the Indian Ocean, obstructs their progress and packs them into a belt of from 100 to 300 miles in width, which, however, increases as it nears Australia until its breadth sometimes becomes so great as to occupy the entire space between the North-West Cape of Australia and the south coast of Java. As a general rule, the monsoon does not blow on the coast of Australia in its full strength until towards the close of the year. It almost invariably sets in with the new moon, and blows in strong gusts, with an almost constant succession of rain-squalls, during periods of eight or ten days, after which there is often fine weather until the next new moon. These bursts of westerly wind can only be depended on with any approach to certainty during the months of January and February, although they have been known to occur as early as November and as late as April. No data exist for estimating the amount of rainfall during the season, nor has it yet been distinctly ascertained which part of the coast is first visited by the monsoon; but I think it will be found that the north and north-west coasts, from Cape Wessel to the Victoria River, get the monsoon almost at the same moment, and that the Gulf of Carpentaria and the north-west coast about Roebuck Bay are not affected by it until the following month. In fact, this paper must rather be considered as suggestive of points of enquiry that can now be followed up than as pretending to decide any one of them. Perhaps the most interesting point at the present moment is the extent to which the monsoon penetrates the interior, and this will be ascertained as settlement extends. Indeed, the explorers Stuart and McKinlay have already furnished data bearing on the point. This, however, is a subject peculiarly Australian, and I shall best answer the purpose of this paper by confining myself to a record of personal experiences of the monsoon in its more northern phases. I have crossed the belt during the months of December, January, and February eleven times since the year 1832, and have found its average width to be 300 miles. On one occasion, in the month of January, 1845, we carried a clear N.E. trade-wind from Singapore into the Straits of Sunda, where, after a ten hours' calm, a clear cool breeze set in from S.E., which never left us until we reached the Cape of Good Hope. There had been a strong burst of westerly wind, however, during the preceding month, as we found a ship under repairs at the Cocos or Keeling Islands that had sprung a leak while carrying sail to beat out of the Straits of Sunda. I have heard of similar interruptions of the monsoon at the time when it should have been at its height, but they are of comparatively rare occurrence and appear to last only a few days at a time. For the rest my experiences while crossing the belt last year (1864), while on my way to and from South Australia, offer so fair a sample of the monsoon, both during its milder and harsher phases, that I cannot do better than give the details. January 22, 1864. Left Ceylon in the steamship Northam, and carried a steady N.E. trade-wind as far as the equator, where the wind became light and variable and the weather cloudy. In lat. 5° 30′ S., we picked up a strong W.N.W. breeze, which we carried for 24 hours, with one heavy downpour of rain on the forenoon of the 26th. At daybreak on the 27th we had light puffs from the southward, and in the afternoon we got the S.E. trade-wind, being then in lat. 11° S. The width of the belt of variable winds will, therefore, have been more than 600 miles. The monsoon was unusually strong this season, as during the preceding month (December 11th, 1863) a heavy westerly gale had washed away part of the sea-wall of Koepang, at the south end of Timor; while the whole of January was stormy, and from the 1st to the 6th of the month of February following a heavy gale from the westward blew down part of the town and deluged the country. In December the same year (1864) I again met with the monsoon while on my passage from Adelaide to Batavia in the ship Omagh. We carried the S.E. trade-wind, clear and steady, until we sighted Java Head on the morning of the 2nd December, and at the same time saw a dense pack of lead-coloured clouds to the N.W. The breeze now fell light, and was variable during the remainder of the day, but mostly from the southern quarter; notwithstanding which a portion of the pack extended itself to the S.E., and the whole horizon to the southward was obscured in the afternoon. Soon after sunset we ran into the pack with a light S.W. wind, and a downpour of rain commenced which lasted almost without intermission until daybreak the following morning. We were by that time about 30 miles to the northward of our position the previous evening, but the pack must have been making its way to the southward with even greater rapidity, for its northern verge was well to the south of Prince's Island, at the entrance of the Strait. A remarkably clear day with a light S.W. wind followed, which carried us through the Strait into the Java Sea, when N.W. and westerly winds brought us to Batavia. On the 16th of the month (December), I left Batavia for Singapore in the mail steamer, and we found the N.E. trade-wind, blowing clear and fresh, 80 miles to the north of Batavia Roads; but by this time the core of the monsoon must have reached, or nearly so, its southern limit. You will have heard by this time at what date the monsoon reached the Adelaide River. Probably not until the latter part of December, for you have advices to the 8th of that month, and the monsoon almost invariably commences at the change of the moon. I trust it will not be long before you have a practical observer in the Northern Territory. There are so many points of interest that can only be cleared up by a systematic series of observations. One of them, namely the distance to which the monsoon penetrates the interior, is almost untouched. The floods met with by McKinlay, towards the end of February, 1862, in lat. 25°, long. 140°, must have been caused by the monsoon rains, but there exist no data at present for determining whether the pack of rain-clouds was forced up the valley of the Victoria River, or whether it came by the way of the Gulf of Carpentaria, where the rainfall is tremendous during those seasons in which the N.E. trade-wind of the Pacific is sufficiently strong to drive the pack well into the Gulf. Indications of heavy rain having recently fallen were met with by McDouall Stuart during the month of March of the some year to the N.W. of the flooded country of McKinlay, and eight degrees further to the westward, but the same indications may have existed between it and the Gulf of Carpentaria had they been sought for. As the periodical rains occur during the season in which the sun is in the southern hemisphere the temperature is high, the thermometer ranging from 76° to 96° Fahr.; but the weather is not felt oppressive by those who are well housed and are not subjected to very active exertion, and as the bracing season sets in immediately after the cessation of the rains labourers do not feel the heat as much as might be expected under the circumstances. The fact that the hot winds of the southern colonies occur during the season of the monsoon, and that both the hot winds and the monsoon winds blow in the same direction, naturally suggests a connection between the two. Indeed, I think it more than probable that when your investigations are completed it will be ascertained that one is a continuation of the other. After the rain-clouds have discharged their moisture to an extent below the point of saturation they will continue their onward course with increased speed and with a sufficient amount of aqueous vapour to prevent the radiation upward of the heat from the earth's surface. Under such circumstances a hot wind would be the natural consequence. It will be necessary to prove that

aqueous vapour actually exists in the upper strata of the atmosphere, and to effect this a series of observations must be carried out at the summit of some isolated hill of sufficient elevation while a hot wind is blowing on the plain below.

The Cool Season (May, June, and July).—The trade wind revives the moment the rains cease. Indeed, when the monsoon is not very strong, it often blows for days together during the intervals that occur between the bursts of westerly wind and rain. During March and April the trade-wind is light, and the breeze refreshing rather than bracing, but during the three following months it sweeps over the entire region in its full strength, and produces the "bitterly cold" weather so often mentioned by your explorers as experienced on the tablelands of the interior. Certain spots upon the coasts of the tropical region are much colder than others during this season, as the S.E. side of the Gulf of Carpentaria and the head of Van Diemen's Gulf. Both these tracts are situated immediately to leeward of masses of elevated tableland, where the temperature is very low at times. The tablelands of the Cape York and Arnheim Peninsula appear to be nearly equal in extent and elevation, and the temperature, during the trade-wind season at least, is likely to be very much the same. It is a singular coincidence that the tracts lying to the N.W. of both these masses of tablelands should have been occupied almost simultaneously by the colonists of Queensland and South Australia. The meteorological observations made in these localities will be peculiarly valuable and interesting. Cape York, like Port Essington, is too insular in its character to afford equal advantages to observers. You will probably by this time have authentic details about the meteorology of the Adelaide River, and as the expedition arrived there during the cool season you will be better informed than I can be as to the range of the thermometer there. The sky is generally without a cloud during this season. Ground fogs are common in the early morning, which usually disappear before 9 o'clock in the forenoon; and it seems probable that in the interior, more especially on the tablelands, very heavy fogs occur at times, for the Port Essington settlement was occasionally enveloped in fog for two or three days in succession, these phenomena taking place during May or June, and being always accompanied by a S.E. wind. The above remarks do not apply to the N.E. coast, where the trade-wind blows in from sea, and is necessarily charged more or less with moisture—so much so, indeed, that in the southern part of New Guinea and in the neighbourhood of Ceram this is the season of the heavy rains, the clouds being intercepted and condensed by the high mountain range which extends along the centre of New Guinea throughout its entire length. And the westernmost corner of the tropical region, between the N.W. Cape and the meridian of Roebuck Bay may be found an exception to the general rule, as the westerly gales of the southern hemisphere sometimes extend a little within the tropic during the winter season.

The Hot, or Electric Season.—This season usually commences in August, and is at its height during the months of September, October, and November. The trade-wind now becomes irregular, and seems to cease altogether in the far interior. To the north of the continent, in the sea lying between Cape York and the east end of Timor, the trade-wind continues to blow steadily, but with diminished strength, from nearly due east, until it is stopped by the monsoon towards the close of the year. This occurs only in mid-sea, for throughout the coast, from Melville Island to Torres Strait, the sea-breeze blows in during the day from N.E. or N.N.E., while at night there will be a light breeze off the land. The weather on the coast continues hazy until the close of the fine season, but it happens very rarely indeed that the aqueous vapour is sufficiently great to produce condensation. On the N.W. coast, again, the trade-wind curves round to the N.W. Cape, and blows along the coast towards Melville Island until the west monsoon sets in. This branch of the trade-wind differs considerably from that of the north coast. It blows with less strength, and is liable to interruption by the heavy squalls from E. and N.E., which will be noticed in a separate paragraph. However, it often blows steadily for 10 or 12 days at a time from W.S.W. to W.N.W., extending more than 200 miles from the land, and blowing well into Timor Straits, and even into Van Diemen's Gulf. This branch of the trade-wind may be supposed to supply the sea-breezes on the coast from N.W. Cape to Cape Londonderry. There are, however, some peculiarities in the sea-breezes of the N.W. coast, between the Victoria River and Van Diemen's Gulf, which seem to point to an independent source. In this neighbourhood the temperature of the atmosphere while the sea-breeze is blowing is some degrees below the temperature of the trade-wind either on the N. or on the N.W. coast, and its purity is so great that a cloud is rarely to be seen. Under these circumstances, I conceive that the sea-breeze on this part of the coast must be derived directly from the current of trade-wind passing overhead. But whatever may be the cause of its superior freshness and elasticity, it will not be disputed that this strip of coast enjoys a climate superior to that of any other portion of the tropical region. In the interior the trade-wind seems to cease altogether at this season, the calm weather being only interrupted by frequent thunderstorms; but that the trade-wind still blows overhead is evident from the fact that the northern half of the sea lying between the N.W. coast and the island of Java, from Sandalwood Island westward, is swept by the trade wind from S. to S.E. up to the end of the year. Indeed, I have been told by natives of the Island of Saru who have lived at Manili, a settlement of theirs on the south coast of Sandalwood Island, that the trade-wind often springs up and blows steadily for days in succession during the intervals between the bursts of the monsoon when at its height. Our position at Port Essington, being on a peninsula jutting out from the mainland, was not favourable for observing the meteorology of the interior. The indications of electrical activity during this season were, however, unmistakable. At night the southern horizon was almost constantly illuminated by flashes of sheet lightning, and the land-breezes either ceased altogether, or came only in the form of thunderstorms. Your opportunities for observing on the mainland will be so much more favourable that it will not be necessary for me to do more than note the phenomena that struck me as being most deserving of further enquiry.

1. Thunderstorms.—The valley at the head of Van Diemen's Gulf appears to be a perfect vortex of electricity. The following extract is from Dr. Leichardt's published Journal, when on the tableland of Arnheim Peninsula (Journal, November 14th, 1845):—"During the night thunder-clouds and lightning were seen in every direction, and the whole atmosphere appeared to be in a state of fermentation. . . . . Thunderstorms formed to the southward and northward, but we had only a few drops of rain. It was remarkable to observe that those to the southward veered round to the S.W. by W., whereas those to the northward veered round to the N.E. and E." A tendency to gyration in the same direction noticed by Leichardt was often observed in the thunderstorms of the Coburg Peninsula, and it excited some speculation, as the cyclones of the southern hemisphere rotate in the same direction, or with the hands of a watch. If future observation should confirm this view, the point will materially aid the enquiry as to the origin of cyclones.

2. Cyclones.—The so-called "hurricane-squalls" which are well described by Captain Wickham in the Appendix to the "Voyage of H.M.S. Beagle," are not rotatory in their character, the wind varying only a few points, usually from E.S.E. to E.N.E., during the continuance of the squalls. Their violence, however, indicates the strength of the electric force generated in the interior of the continent, and it is possible that they might assume a rotatory character if they met with obstruction in their course, such as the pack of rain-clouds which forms the core of the westerly monsoon. But I merely offer this as a suggestion, for I have no proof whatever that such actually occurs. Only two authenticated cases of cyclones taking place in or near the tropical region can be produced, and both happened in the year 1839—one in the month of February and the other in November. Trepang fishers sometimes reported whirlwind storms near Cape Londonderry that had sunk or disabled part of the fleet. Ships sailing to the southward from Bali Strait during the westerly monsoon have returned dismasted, or have disappeared altogether, and about four years ago a cyclone coming from the eastward devastated the Cocos or Keeling Islands in the Indian Ocean, but their origin has never been traced. It fortunately happened that two very accurate observers witnessed the Sharks Bay cyclone of February 28, 1839, and that of Port Essington on the 25th November the same year, namely Captain (now Sir George) Grey, and Captain Owen Stanley, R.N., and their notes are given in works that you must have in the library—Captain Grey's Narrative of his Expeditions, and Captain Stokes's Narrative of the Surveying Voyage of H.M.S. Beagle. Captain Grey's cyclone was experienced at Dorree Island, in lat. 25° S. It commenced at S.E., and after blowing with great fury for some hours, shifted to the opposite direction (N.W.) and blew with equal violence for nearly the same period. According to the rules of the "Law of Storms," this cyclone must have been moving towards the S.W., and Captain Grey's party was near the centre. The fact of its occurring at the time when the westerly monsoon reaches its southernmost limit supports my suggestion that these cyclones gyrate along the southern edge of the pack of rain-clouds, from east to west. The Port Essington hurricane, which is fully described in Captain Stokes's work, also occurred at the time when the southern edge of the monsoon was close at hand. Captain Stokes mentioned that the "French discovery ships," meaning Commodore D'Urville's squadron, the Astrolabe and Zelee, met with this cyclone in Torres Straits. I have never had an opportunity of verifying this statement, but it ought to be done. The Commodore was at Port Essington in April that year, and sailed for the Moluccas towards the end of that month, and thence to New Guinea and Torres Straits; but an impression remains on my mind that he left those seas some months before the end of the year. I daresay you have often observed how small matters of this kind interfere with the progress of enquiry. I have a lively recollection of the perplexity caused in my mind for years by the Burning Islands laid down in Torres Strait and on the N.W. coast of Borneo, and which have only lately been struck out of the maps as not existing.

Ozone.—This principle has been discerned so recently that I should probably have avoided reference to it on the present occasion had I not been aware that you are well versed in the subject, and fully alive to its importance. Indeed, it was at your observatory that I first saw the test-papers and learned the mode of taking observations. When it is considered that the discovery of ozone resulted from an analysis of the peculiar odour created by a discharge of electricity, and that artificial ozone can be produced by manipulating a Leyden jar, its intimate connection with atmospheric electricity can scarcely be doubted. I anticipate the most important results from your ozone observations in the Northern Territory. You will probably find it necessary to prosecute a set of observations distinct from the 24 hours' register, to ascertain the amount of ozone in each particular wind, whether trade or monsoon wind, land or sea breeze, thunderstorm or cyclone. I presume that the peculiar property of ozone in neutralizing malaria has been fully recognised in scientific circles at home, from the fact that the Royal Observatory has been at the trouble of distributing the test-papers throughout the world. I often amuse myself by carrying on a set of "retrospective" observations as it were, calling to mind all the local circumstances of spots I have met with in this part of the world which are famed for their unhealthiness. I may have made a good hit in my little book on Port Essington (1846), where I have suggested that the comparative salubrity of the inner harbour during the earlier years of the settlement was attributable to the cyclone. The prominence that I have attributed to ocean tides as antagonistic agents to malaria will, I think, still hold good, for the action of the tides promotes circulation of the atmosphere, and without this ozone could not be distributed.

GEORGE WINDSOR EARL.

Andrews, Thomas, & Clark, Printers, Register and Observer Offices, Grenfell Street, Adelaide.